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MIMETIC POLYMORPHISM, A CONTROVERSIAL CHAPTER OF DARWINISM (*Concluded*)

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C. The model of other modifiers

FISHER further considered, as a good model of what happened in the evolution of mimicry, the famous case of the inheritance of the hooded pattern of rats, as studied by Castle. A single Mendelian difference decides between hooded or not hooded, but modifiers can be selected in both directions which shift the pattern towards almost complete pigmentation or depigmentation. At one point of the discussion Fisher uses the words: "Unless the above analogies (sex and hooded rats) are wholly misleading . . ." (1927, p. 275), which might mean or not that he did not feel completely satisfied with his examples. I think that the present example is just as misleading as the former one. Nobody denies the possibility of a change of pattern by selection of modifiers. But in this case, as in so many others, the modification is of a simple linear order. The pattern determined by the allele in question varies quantitatively under the influence of modifiers (also environment in many cases) by producing more or less pigment, which spreads or retracts according to the inherited pattern. But this is not the way the pattern does change in mimicry, where completely new pattern elements, and also shapes of scales and different pigments combine to produce the new pattern. (Only certain small variations, such as those described by Ford for *P. dardanus poultoni*—see above—can be compared with the rat case; these produce small plus or minus changes of spot within the mimetic pattern.)

Ford (1940a, p. 236) used a different model, actually taken from Lepidoptera, as mentioned above. Besides his own work on *Abraxas*, dealing only with intensity of color, he cites especially the work of Federley (1920) on *Spilosoma lubricipeda* L., an arctiid moth, which has yellowish wings with small black spots. A melanic variety, called *zatima*, has practically black wings. The F_1 hybrid between both is intermediate (Ford calls it incomplete dominance of black) and F_2 segregates 1:2:1 showing a simple Mendelian factor difference, as Standfuss (1896) already knew (see Figs. 3a, 3b, 4). Later authors found modifiers for this pattern. Thus Federley found multiple modifiers as a chance find in the single pair from which all his material was derived. Goldschmidt (1924) working with material of different origin found a different genetic situation (see below). The modifiers in Federley's material shift the degree of darkness upon the wings in both directions (see Figs. 3a, 3b, 4). Heterozygotes *Zz* can show a considerable variation, according to the presence and segregation of these modifiers. By selection of the darker and lighter phenotypes, the homozygotes *ZZ* and *zz*, *zatima* and *lubricipeda*, could also be obtained in combination with plus or minus modifiers and by continued minus selection in *ZZ* almost light individuals could be obtained, while in *zz*, *lubricipeda*, the dark spots can be made to

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disappear almost completely. These selected types transmit their characteristics to the offspring. This case is then clearly a parallel to Castle's rats, and the same objections against a comparison with the mimetic pattern must be raised.

But Ford thinks that this example has a still greater importance for the neo-Darwinian theory

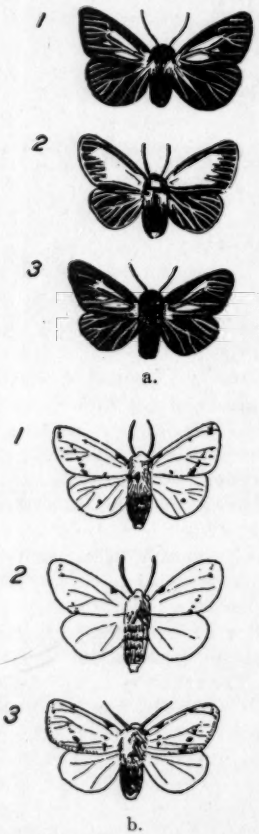

FIG. 3. SPILOSOMA LUBRICIPEDA
a. Types of *zatima* with modifiers. b. Types of *lubricipeda* with modifiers.

of mimicry. He emphasizes that the multiple modifiers of Federley affect the heterozygote *Zz* more than the homozygous *zatima ZZ* and least the homozygous *lubricipeda zz*. "There is a suggestion therefore that the wild type form is less susceptible to the effect of genetic variability than are the heterozygous and homozygous varieties. This might be anticipated on theoretical grounds. Any successful species should be so adjusted that its usual environment represents a near approach to its optimum. That is to say, the genes ordinarily present should be selected in such a way as to produce their most favorable effects in the conditions to which a species is normally exposed. Genetic variations in the effect of wild type allelomorphs are therefore especially liable to be disadvantageous. Accordingly, we may anticipate that the reactions to which such genes give rise

FIG. 4. DTO. F_1 TYPES WITH VARIATION BY MODIFIERS
Redrawn from photos by Federley, 1920

will have been so 'buffered' by selection that segregation within the gene-complex will produce a minimum amount of variation in the characters which they control. No such adjustment will have taken place in respect to rare varieties subject to elimination. We may therefore expect to find the latter less stabilized in respect of genetic variability than their normal allelomorphs, a condition which appears to be fulfilled" (Ford, 1940a, p. 236).

We have quoted Ford's argument in spite of the fact just stressed that the *Spilosoma* case has no bearing upon mimetic resemblance, because here Fisher's ideas are applied to a specific, rather well-known case, ideas which *mutatis mutandis* are the same as applied to mimicry. I believe that I can

show that the neo-Darwinian explanation, just presented, is, indeed, very far-fetched and unnecessary. (1) Federley secured his modifiers only by selection from the heterozygote *Zz*, where alone he could originally discover their action. Thus we do not know whether they were introduced from *ZZ* to *zz*. All his material is descended from one pair. (2) The segregation from selected F_1 individuals showed a shift of the amount of dark pigment both in *ZZ* and *zz*. (3) Ford is much impressed by the fact that the *lubricipeda* type (*zz*) is less affected by the modifiers than the *zatima* type *ZZ*. If we look at the actual specimens we can easily see why, without appealing to theories of selection (see Figs. 3a, 3b, 4). *Lubricipeda* is characterized by a group of black spots upon a yellowish background, which form a group of rudimentary broken bands characteristic of the basic pattern of the group. The *zatima* factor produces an excess of pigment deposited almost in all cells of the wing (and also in body hair), thus covering completely the *lubricipeda* pattern. In *Zz*, this extra pigment encroaches more or less upon the light background, starting from the posterior edge of the wing. The modifiers which Federley discovered in the heterozygote (they were found only in a smaller degree in Standfuss' and my own crosses) increase or decrease the amount of pigment "flowing" over the wing. In the intermediate F_1 there is much room on the wing both for encroachment of black upon white (yellowish) and for restriction of black in favor of white. In the homozygote *ZZ* much restriction from the almost black wing is possible, but very little room is left for extension. In the wild type homozygote there is no pigment between the veins to be modified in the absence of *Z*. The modifiers therefore are either left in a void, or might act upon the small black spots of the rudimentary bands. Actually Federley found the latter. Now assume that the modifiers of a definite combination restrict the *ZZ* action by one-half. A half black wing is indeed very different from an all black one. But if the same modifiers restrict a tiny spot in *zz* by one-half, the visible change in the spot amounts to a very slight change of the total wing. The observed facts thus are simple spatial consequences of the pattern involved. If we do not look at the facts with the intention of proving a neo-Darwinian mechanism of evolution, they are simple enough and do not require any explanation of the complicated type just presented. This critique again may serve as a model for the mimicry cases. (4) There is no reason to assume that the pattern of the rarer variety is less stabilized than that of the original species. *Zatima* is actually the preponderant form in certain regions (e.g., the Friesian islands). Here the pattern is found as described (*zatima*), but also completely dark forms, called *deschangei*, occur. I was able to show that this type is based upon a multiple allele of *Z* (Goldschmidt 1924). In my material a single modifier could be seen at work, which acted, on a lower level, like those of Federley. The genetic results indicated that the modifier was present also in the wild form, but as it acted only so little upon the all dark wing it could hardly be expected that its action upon small dots would be distinguishable. Thus, all in all, I believe that the elaborate theory of selection presented by Ford for this case is completely unnecessary. Actually, wherever characters are studied which may vary in a plus or minus direction, modifiers are found which influence the phenotype. When we analyze such cases from the standpoint of developmental dynamics, we rarely find difficulties in principle for an interpretation of the resulting phenotype. There is no doubt that, under definite circumstances, such modifiers may be selected for one or the other end of the possible series of phenotypes. But this does not prove that a mutant phenotype which is adapted to certain conditions of habitat must have evolved by selection of such modifiers, if actually the mutant itself was sufficiently good as an adaptation. Such frequent types of variation found in nature as melanism, leucism, xanthism, rutilism are known to have occurred under laboratory conditions as ordinary mutants, with the maximum effect accomplished in one step. Therefore nothing else is required in nature either.

D. Non-mimetic polymorphism

The last sentences lead us to the general problem of polymorphism, which has been frequently discussed together with mimetic polymorphism e.g., by Ford (1940b), Goldschmidt (1940) and Huxley (1943). We point to these books for the detailed material and mention only such facts as are relevant for our present discussion. Ford (1940b) has subdivided polymorphism into four groups: (1) disadvantageous varieties eliminated by selection and maintained at a low level by recurrent mutation of the genes controlling them; (2) variations due to the effects of genes approxi-

mately neutral as regards survival value; (3) those dependent upon genes maintained by a balance of selective agencies; (4) advantageous varieties controlled by genes spreading through the population and displacing their alleles. Only the third and fourth groups are regarded as polymorphism proper; here two or more well-marked forms, capable of appearing among the offspring of a single female, occur with frequencies high enough to exclude the maintenance of the rarest of them by recurrent mutation. It is clearly Ford's third group to which the material under discussion belongs; Ford calls it *balanced polymorphism*.

But the fourth group mentioned by Ford (termed by him *transient polymorphism*) is actually the one which is most suitable to shed light also upon the third one, because here cases have been studied in which both contributing agencies, the genetical basis and the action of selection, are known. These are cases of melanism (including nigrism) in Lepidoptera. Ford (1937, p. 478) has assembled the literature on the subject of melanism —including those cases to be discussed here as well as others—and we point to his review for the details and literature. (Another discussion is found in Goldschmidt, 1940). In a general way we might say that melanic varieties falling under the above group 1 are frequent. In individual cases they may be based upon simple recessive, dominant, or multiple allelic mutants, or also upon a few collaborating or additive multiple factors. But the really interesting cases are those of so-called industrial melanism, which means the observed spread of melanic varieties in industrial districts of England and Central Europe within the last sixty years. These are the cases which comprise what Ford calls transient polymorphism. In Ford's just mentioned review all cases are assembled, and we might say, generally, that the majority of those studied genetically show that melanism is based upon dominant mutation, sometimes with multiple alleles or with additional multifactorial modifiers. I think that the best observed case, as far as the spreading of the mutant is concerned (because of its very recent first appearance at a time when attention already had been drawn to the phenomenon), is that of *Cymatophora or* (in the region of Hamburg as studied by Hasebroek). Here a simple Mendelian difference is present (as I was able to confirm in a few breeding experiments). But this form does not occur in great numbers and, therefore, cannot be observed in massed populations. Such a study is possible in the nun-moth *Lymantria monacha* L., a dangerous forest pest, appearing suddenly by millions of individuals and therefore closely watched by foresters and entomologists. Therefore much information is available upon the population side of the problem, appearance, disappearance, spreading, etc. Likewise, the genetics is well known. Melanism is mainly based here upon a sex-linked dominant (Goldschmidt, 1921). But there are also less dark types, produced by two pairs of autosomal alleles, and the maximum effect is produced in combinations of all three. It is known that the different dusky and dark forms were rare collector's specimens about a hundred years ago, which means that the three mutants (all three?) appeared from time to time but could not establish themselves. During the industrial era, and within industrial regions, this condition was completely reversed, so that now the populations contain a majority of dark forms. My own laboratory work began with thousands of pupae collected in one infested area in an industrial region. This population contained an overwhelming majority of the melanistic recombinations (grey to black) of the three alleles, while the original, recessive, white form was rather rare. But obviously it still segregated in the population. It is generally agreed that here selection was at work, and a suggestion, made both by Harrison and myself, is that the melanic mutant conferred, as a physiological by-effect, upon the caterpillars a resistance to ill-effects of feeding upon leaves incrusted with metal salts. But this case shows also how careful we must be in handling questions of selection. In the huge populations of the nun-moth found at the peak of an outbreak the selective or non-selective destruction brought about by the main known agencies does not affect the imago. More than 90 per cent of the caterpillars succumb to virus or bacterial diseases (polyhedra and flacherie). The next powerful selective factor is afforded by the larvae of beetles feeding on the egg clusters. Next come predatory beetles (like *Calosoma*), and next some birds that feed on hairy caterpillars. This shows that whatever selective advantage the melanistic mutant confers upon the imago does not work at all in the huge populations at the summit of an outbreak. But usually such pests subside after a few years, and in the interval between two outbreaks the animal becomes utterly rare, so that special attention by foresters is required to find out where it survives at all. It is in this stage of

very small and restricted populations, in which disease, etc., have not much chance to be selective agencies, that the eventual small advantage conferred by the physiological adjunct of melanism, whatever it is, will work towards supplanting the original form by the melanic one. I think that this group of facts, a group in which more detailed knowledge of genetics, ecology, and working of selection is available than anywhere else, demonstrates that selection can work very fast if new situations appear and that no slow accumulation of modifiers is needed to produce the result which is reached by single mutational steps either completely or almost completely. There is no reason for supposing that cases of mimicry in which only the mimetic type exists, i.e., without polymorphism, must have originated differently.

But let us now return to Ford's group of balanced polymorphism, which comprises also the mimetic polymorphism. This type of variation is very frequent, as the examples compiled in recent books (e.g., Goldschmidt, 1940; Huxley, 1943) show. Differences in detail are found between the cases, but as to general principles the facts are comparable. We are not concerned here with the selectionist side of the problem, viz., which conditions have to be fulfilled in order to keep a polymorphic form in rather stable equilibrium. Fisher (l.c.) has worked out the statistical and ecological requirements, and those later authors who discussed this subject have accepted his analysis. The details do not concern us here, because they apply generally to balanced polymorphism whatever its genetic basis. But the genetic side is important, as it might throw light, by analogy, upon the cases of mimicry. In a general way we might say that such examples of polymorphism as have been studied genetically show that the differences between the different forms are based either upon single Mendelian differences—e.g., *Colias* (Gerould, 1921), *Argynnis* (Goldschmidt and Fischer, 1922)—or upon multiple alleles—e.g., lady beetles (Lus, 1926)—or upon ordinary Mendelian recombinations—e.g., *Callimorpha dominula* (Goldschmidt, 1924), *Helix* (Coutagne, 1895/96; Lang, 1904 ff). Using one of these examples we can devise a good analogy to mimetic polymorphism.

Already in pre-Mendelian days Coutagne (1895) found that different colonies of the garden snail *Cepaea nemoralis* contained an assembly of types, typical for each colony, but different between them. These types are based mainly upon the ground-color of the shell, the number and thickness of the bands, the interruption or confluence of bands, etc. Coutagne realized that these could be understood as permutations of a number of basic variants and actually described them in terms reminiscent of Mendelian symbols. In addition, he realized (p. 402) that different colonies had different features in other quantitative characters which are difficult of appraisal. We should call these differences today (micro-) geographical races, though an additional purely environmental effect cannot be excluded. Furthermore, Coutagne emphasized that these local characters—aside from polymorphism—were found in strikingly parallel expression in other species of snails living in the same locality. Here, then, we have a perfect analogy to the *Papilio* cases: local polymorphism, with different relative numbers of the types from locality to locality, including also localities without polymorphism; further geographic variation from place to place; and also parallelism of geographic variation with that of different species. (Recent work with similar material is reviewed by Diver in Huxley, 1940.) Later, Lang (1904 ff.) showed that a number of the individual variants were based upon simple Mendelian differences, e.g., complete bands, interrupted bands, number of bands, and union of bands, and that recombinations of these characters could be bred.

One of these recombinations permits us to invent what we consider to be a beautiful analogy to mimicry. Shells of snails may also exhibit mimicry in certain cases. Probably the most beautiful case of mimicry I have ever seen (and I have had the good fortune to see many in different climes), I found by chance when, as a young student, I was working at a Mediterranean marine laboratory. I was drawing an alcyonarian colony of special beauty, having a carmine red flesh with yellow polyps that, when retracted, appeared like yellow stars on the carmine background. Suddenly I noticed that one or two of the polyps seemed to change place, and observing more closely, I found a small snail (genus *Ovata*?) crawling slowly over the colony. Its shell had exactly the carmine color of the alcyonarian, and yellow star-like spots imitated perfectly the retracted polyps. Now to our invented analogy. Lang combined the Mendelian alleles for complete black color by fusion of the five bands with the factor for interruption of bands (dotted bands) and thus obtained cross-striated shells (Fig. 5).

It is well-known that such zebra-stripes form a powerful concealing coloration, or, in other cases, a frightening pattern (see Cott, 1940). Let us now suppose that such a shell were found in nature and in a habitat in which the zebra-pattern was actually a concealing pattern, or in association with unpalatable forms with a warning pattern of stripes; we could certainly point to a beautiful case of mimicry. Now the colony in question would segregate, if heterozygous, at least two types of non-mimetic snails besides the mimetic zebra-striped one, and a genetic analysis would discover only simple Mendelian relations. The neo-Darwinian, assuming that Lang's work did not exist, would tell us that such a huge departure from the typical longitudinal stripes of the nearest relatives of our snail as a zebra pattern, and this in imitation of a different unpalatable family,

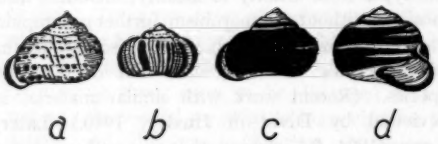

FIG. 5. CEPAEA NEMORALIS, MUTANTS FOR DOTTED (a) AND CONFLUENT (c, d) BANDS MAY COMBINE INTO CROSS STRIATION (b)
After Lang, from Goldschmidt

could only have been produced by a slow selection of modifiers for a zebra pattern—in spite of the simple Mendelian behavior of the patterns.

I do not mean to imply that the analogy could be carried on indefinitely to perfection. It is only an analogy. But it shows, I think, that a knowledge of standard cases of polymorphism makes us expect that mimetic polymorphism also originated by simple mutation or mutations of the type which Fisher calls saltation.

Thus far, we have not taken into account the fact that mimetic polymorphism occurs, in the majority of cases and in different genera and families, combined with the sex-controlled type of inheritance. This might be nothing but a matter of chance. In non-mimetic Lepidoptera, the polymorphism does affect both sexes or is sex-controlled, affecting only females. In the latter cases the deviation of the second type from the male type is not larger than that which we usually associate with a Mendelian mutation (e.g., in *Colias, Argynnis*). As some of the mimics are near relatives of these forms in which ordinary sex-controlled dimorphism exists, we might assume that a genetic constitution is present in these groups which makes mutants liable to be sex-controlled. Thus the sex-control is independent of the origin of mimetic resemblance. But we cannot help being surprised that in these cases male sex-controlled mutants or such as would affect both sexes equally or would suppress the sex control did not appear and be selected, thus providing protection also for the males. The genetic possibility of all these expected events cannot be denied. There exists a case of a moth (*Nemeophila plantaginis*) where male dimorphism (without female dimorphism) exists. Unfortunately the genetics are not clear, though the few data available (some of my own) suggest a simple Mendelian difference. Further, in cases in which a considerable sexual difference exists, mutants occur which affect both sexes equally, others which affect only the male wings, others affecting female wings. Examples of all three types have been described in *Lymantria dispar* (review in Goldschmidt, 1934), and here recessive mutants as well as multiple alleles affecting different parts of the wing pattern have been found. Thus it remains a remarkable fact that sex-control of mimicry patterns is such a frequent occurrence. I cannot help feeling (and I have emphasized this before in Goldschmidt, 1913, 1920a, 1927) that there is a relation between the features of embryonic determination which makes for sex-controlled inheritance and the type of mutation (or saltation) which is involved in the production of the mimetic pattern. But this is a problem of developmental genetics, to which we shall return below.

E. Multiple effects

Both Fisher and Ford emphasize over and over again that the mimics differ from their non-mimetic males or their relatives not only in regard to wing pattern, but also in a series of other characters which are supposed to be part of the adaptation, but cannot be conceived as multiple effects of one mutant allele. Such characters exhibited in model and mimic are the form of the wings, especially the presence or absence of tails; the general ecology; the mode of flying; the presence of terminal knobs on the antennae and in palpi of the same color. Fisher considered the last characters so important that he devoted a colored plate to them. Such features, then, are considered as proof that the mimetic resemblance has been accumulated slowly by selection.

Let us begin with the tails. Nothing is known about the inheritance of tails in Lepidoptera. But we know (Süffert, 1929a) that in the development of the tails of swallowtails, first a normal wing edge is formed and secondarily cut out into the tailed shape by degeneration of peripheral parts. This fact makes the formation or non-formation of tails a rather simple embryological event, i.e., degeneration or no degeneration, which we may expect to find under simple genetic control. There exists one study of the genetics of wing-shape in Lepidoptera, that of Henke (1937) for the flour moth, in which lines with broader or more pointed wings exist. This shape, based upon differential growth, is already determined and visible at the time of pupation. Henke found that this growth is controlled by a single Mendelian pair *Pp* of which *P* mainly produces a shortening of the wing and a broadening and enlargement of the forewing. In *pp* the wingtip is more rounded. But here also a process comparable to that found by Süffert is encountered: the imaginal wing is not identical with the pupal wing, but part of its edge is cut out from the pupal wing by degeneration of peripheral parts (as described by Köhler, 1932). The allele *p* produces in the forewing a line of "cutting out" which is different from that in *P*; namely, a larger part of the pupal wing remains in regard to length, not in regard to breadth. Here, then, we see at work a type of developmental process comparable to that which produces tails in *Papilio*; in the flour moth this process is controlled by a single pair of alleles. One might add that in *Drosophila* single alleles are known which change wing shape in different ways, e.g., like the mutants expanded, the bran alleles of arc, and also certain loci like dumpy which produce changes of the wing edge (truncation) that closely resemble a formation of tails, including features of development (as studied by Auerbach, Goldschmidt, Waddington).

Also, an example from the Lepidoptera can be mentioned which involves sex-controlled inheritance, as was the case in the Papilios. The female and male of *Lymantria dispar* have wings of completely different form. The neo-Darwinian interpretation would be that a series of modifiers for these forms has been accumulated by selection, and some of these modifiers react only with the female, others only with the male genotype. But in intersexuality the typical shapes can be completely reversed. This happens at a definite grade of intersexuality; in intersexual females already in medium intersexuality, in intersexual males only in high intersexuality. There is no reason for advocating any modifiers if the facts can just as well be explained by one single genetic background with alternative reaction.

In the mimetic Papilios we actually face the following types:

1. *P. polytes:* Males and all three females with tails, also models with tails.
2. *P. dardanus:* Males with tails, females and models without tails, but one, *hippocoon*, sometimes with rudimentary tails.
3. *P. lysithous* (from Rio Grande de Sul; see Jordan, 1911), thus far not mentioned, with different females mimicking members of the Pharmacophagus group; three types of mimetic females with tails, two of the models tailed, one without tails.
4. *P. memnon:* Two non-mimetic females without tails like the male, but one mimetic female with tails like the model.

Thus we find perfectly good mimics with tails when the model is tailless, non-mimics with or without tails, males with or without tails, and females with and without tails whether mimics or not. An explanation, on Fisher's line of reasoning, requires different selections of modifiers for the presence or absence of tails, different reactions with the sexual genotype, and different reactions with the "switch genes." There is an additional need for an explanation in cases of incongruity between model and mimic. In view of the genetic facts indicating a simple control of wing shape, I prefer to assume that the different *A*, *B* mutants, the switch-genes, have multiple effects that either include or do not include the control of the tail-forming process in one or the other direction.

The second so-called multiple feature of the mimetic resemblance is the color of the pigment within the mimetic pattern. It is, in the cases which we are discussing here, a replacement of yellow by white or different reds or brownish. The genetics of a large number of comparable color varieties have been studied in Lepidoptera. Some are known to occur as simple mutants: Many red forms are known to produce simple Mendelian mutants of yellow color, red being dominant over yellow (e.g., *Callimorpha dominula* in N. Germany); yellow forms can produce purple mutants (in Arctiids); other yellows produce white mutants, with or without sex control (e.g., *Colias* with a female sex-control, or *Nemeophila plantaginis hospita* with male sex-control). But also simple Mendelian mutants are known in which the

same color mutant affects also the pattern, e.g., the form *hospita* of *Nemeophila plantaginis* represents a change from reddish yellow to white simultaneously with a shift in the dark pattern. (For other examples see Standfuss, 1898, and Ford, 1937). One recessive mutant in the males of *Lymantria dispar* changes the ground color (not by a change in pigment but by the distribution of white and black scales) only in certain parts of the wing, thus changing the pattern. There is no genetic reason to assume that the pigments found in mimetic females differing from those of the males (and non-mimetic females) are the result of a slow selective process. It is certainly possible that they are based upon single mutations independent of the pattern. But there is no reason to deny that they might be just as well multiple effects of the same "switch-gene." Actually the data on intermediates agree well with this view (see below).

We turn now to the problem of color of the palpi and antennae-tips, though it is not involved in the cases of polymorphism which are in the foreground of the present discussion. The entomologist looks very closely, with a lens, at such small organs as are of taxonomic importance. But shall we really believe that birds, monkeys, or mantids distinguish such minutiae in a flying butterfly? If not, I cannot see how they could have been selected. In addition, what little we know genetically, does not point to an independent genetic determination of these characters as a basis of the assumed selection. It is known for a number of cases that the same mutant alleles in Lepidoptera control the color of different organs. Thus the yellow wing spot mutant of *Callimorpha dominula* has also a yellow striped abdomen, and in the melanistic combinations of the same moth the reduction of abdominal yellow runs parallel to that of wing yellow (photographs in Goldschmidt, 1924). In the melanistic varieties of the nun-moth the color of body hair changes parallel with that of the wings, and this includes also the hair tuft on the palpi (photographs in Goldschmidt, 1921). There can be no doubt that unifactorial control of wing and body pigment exists; therefore, there is no reason why the mutant controlling wing pattern and color should not also control color of palpi or other organs. One can exemplify also with the white and other alleles in *Drosophila*, which control simultaneously color of testis and Malpighian tubules; the same is true for the pale silver alleles (Goldschmidt, 1945).

The last of the so-called multiple mimetic adaptations are the ecological features like preference for the same habitat inhabited by the model, identical mode of flight in both (different from that of congeners), and similar observations. Aside from the fact that nothing is known of the genetics of such and comparable features, this group of assumed adaptations is to be considered as insufficiently established. In literature both pro and contra mimicry, statements of the most conflicting nature can be found, e.g., that the model flies in the plains and the mimic in the woods, or that the mode of flight of the mimic is so different from that of the model that even the dull human senses would not be deceived. Under these circumstances it is better to mark this group of doubtful adaptations as *non liquet*. It certainly is not fit to be used as a decisive argument in either sense.

F. Parallel geographic variation

It is a well-known fact that the models of mimetic forms, when widespread, have formed distinctive geographic races or subspecies, sometimes recognized as such, sometimes called species. If the mimic has the same range as the model it copies its typical local form. We mentioned above such cases in different mimetic species, especially in *Papilio dardanus*. Thus the female form *hippocoon* of West Africa is changed in the East into *hippocoonides* with a larger area of white in the hind wings; this is clearly an imitation of the eastern variety of the model *Amauris nidvius dominicanus* (see Fig. 2). Many such cases are reported in the literature already quoted. But it must be added that geographic varieties of mimics are known for which no model exists. Ford quotes the case of *P. dardanus salaami*, in which in the eastern group all pale markings are orange, though no such model is available. In the western group the form *niobe* is a parallel orange variety, but here a model is present (*Bematistes sellus* Aur.).

Such facts of parallel geographic variation, again, are assumed to demonstrate the selection of modifiers in the origin of mimetic resemblance. Unfortunately, no genetic experiments of crossing geographic races seem available (except one reported by Ford, in which geographic difference was not represented). Thus we are completely left to analogies from other cases. The best analysed case of geographic variation in Lepidoptera is that of *Lymantria dispar*. Here patterns of the markings of caterpillars and, to a

lesser extent, wing colors of the imago vary geographically. In both cases all or the major part of the differences are based upon systems of multiple alleles, i.e., mutants of the same alleles which in all races control pattern and color respectively. Apart from geographic variation, we know that frequently color variations and pattern variations are primarily based upon multiple alleles. Thus the complicated pattern of markings of the silk worm caterpillars is based upon a series of multiple alleles (many Japanese investigators, e.g., Tanaka). In the Tortricid moth *Acalla camariana*, the wing types brown marbled, black, and gray are controlled by multiple alleles (Fryer, 1928, 1931). The same is true for the melanic forms of *Aglia tau* L. studied by Standfuss (1910). It may be added that here ground color, pattern, and certain nail-like markings in the eyespots are involved in the multiple effects of the alleles. What is probably the largest series of multiple alleles controlling different patterns has been described in lady-beetles by Lus (1926). Thus I confidently expect that future research will reveal that the differences between geographic races of mimics (and models) will be of a multiple allelomorphic character, involving the locus controlling the mimetic pattern. I do not doubt that the neo-Darwinians would say in this case that in fact an allele of one of the supposed modifiers reacting with the switch-gene is responsible. Our answer to such an assumption is contained in the previous discussion.

There remains the problem why the subspecies of the mimic resemble the subspecies of the model. The neo-Darwinian would have to assume that each subspecie of the model was first formed as a pre-adaptation to the new habitat by one or another genetical process. The mimic, however, if entering the same region, was subjected to the selection of modifiers which changed its pattern towards resemblance to the new type of model. Let us discuss whether our knowledge of geographic variation warrants such assumptions.

The huge material on this subject has been recently reviewed from different points of view in the books by Dobzhansky, Mayr, Goldschmidt, and Huxley, where details and literature can be found. As we are discussing here a case in the Lepidoptera, we shall lean especially on material taken from this group. Though the adaptive nature of visible geographic variation is not always discernible, a sufficient number of cases has become known in which it has either been proven or is suggested that the visible differential characters, though themselves not adaptive, are the external visible effect of physiological differences which are of an adaptive nature. Many examples for Lepidoptera can be found in the present author's work on *Lymantria dispar*, and among them is at least one involving changes of pattern which might be easily compared with those described for the subspecies of *P. dardanus* (see discussion above). It has been known for a long time that characteristics of geographic races in different groups of animals and plants tend to run in a parallel series, e.g., northern races of different mammals and birds tend to be larger or darker, etc. These so-called rules, e.g., Bergmann's, Gloger's, and Allen's rules for vertebrates or Vavilov's for cultivated plants, have been fully discussed in the repeatedly mentioned books. There can be no doubt that wherever such rules are actually proven the important fact behind them is that in some way mutants of pigment, size, etc., are physiologically preadapted to definite habitats into which these forms are able to migrate. Preadaptation, selective migration of preadapted mutants into definite niches, and the similar physiological background of different but similar-looking mutants furnish the adequate explanation. In the Lepidoptera many examples of all this are known, although no "rules" have been laid down. We might quote the facts of industrial melanism already discussed above. This melanism occurs in such different families as the Geometridae, Lymantriidae, Tortricidae, and Saturnidae. The basic patterns of these groups and the species in question are rather different, and so are the intermediate steps, e.g., of the pepper moth and the nun moth. But the end product of this selection, black wings, is more or less identical in all. There is no reason to deny that similar potentialities are present in the families to which mimics and models belong. Actually Punnett has already discussed a good example. He mentioned the observation, made already by Bates, that in South America "certain color schemes are characteristic of distinct geographical regions . . . where they may occur in species belonging to very different genera and families" (l.c., p. 138) (Fig. 6). This local fashion[1], as one may call it, occurring in Heliconines, Ithomiines, Pierids, and Nymphalines has been claimed to unite simultaneously all these forms into a mimicry ring; but this is more than doubtful. Punnett pointed out, properly I think, and as far as I am aware, for the first time, that it

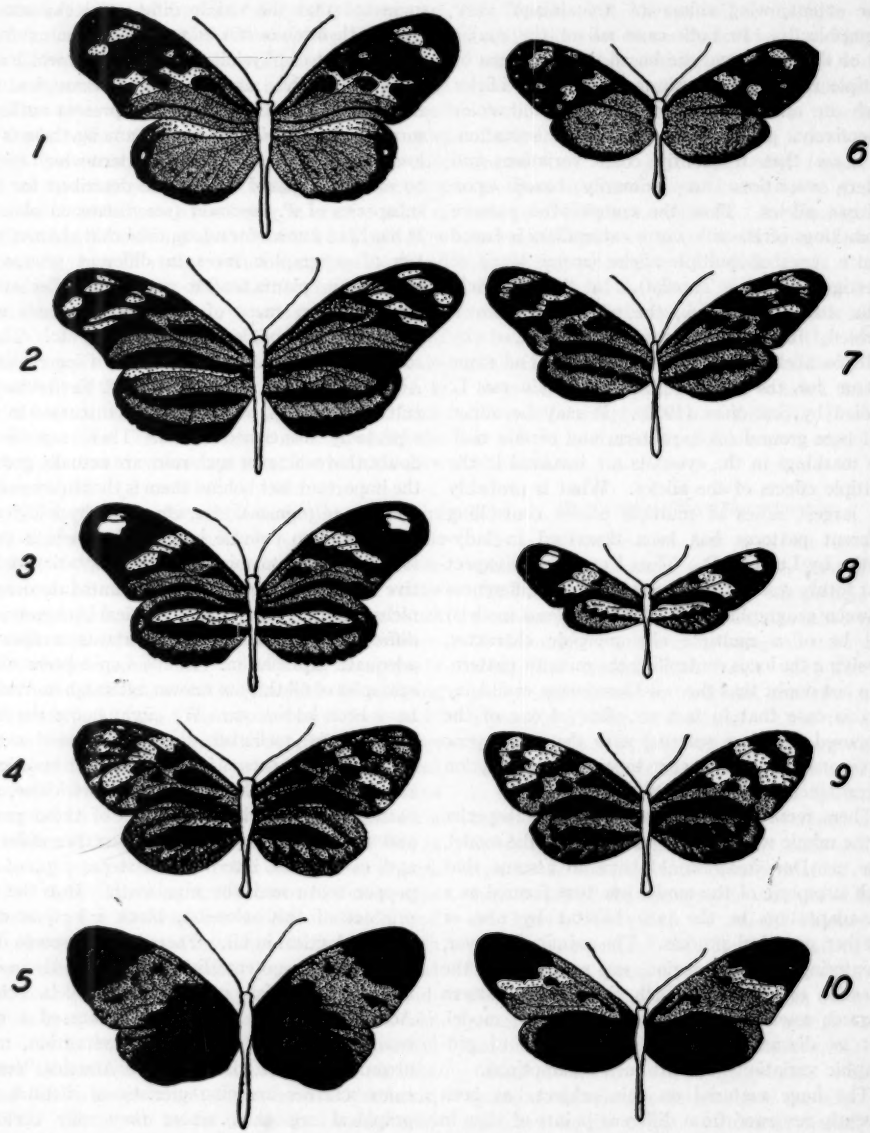

FIG. 6. GEOGRAPHICAL FASHIONS, EMBRACING DIFFERENT FORMS FROM PUNNETT, 1915, PLATE 15 IN COLORS, REDRAWN

Parallel series of patterns in Heliconinae and Ithomiinae.

1. Heliconius mirus
6. Mechanitis elisa
2. H. telchinea
7. M. saturata
} Central America

3. H. eucrate
8. M. lysimnia
} Eastern Brazil

4. H. pardalinus
9. M. egaensis
} Upper Amazon

5. H. splendens
10. M. methona
} Ecuador, Peru, Bolivia

is more probable that the local fashion is based upon some hidden physiological properties combined with definite patterns, i.e., the same interpretation which has been proven to be true in other cases (presented, e.g., in Goldschmidt, 1940).

Returning again to the parallel geographic variation of model and mimic we conclude that the explanation derived for comparable cases not involving mimicry should also cover the situation in mimicry. But this does not yet solve the genetical problem in the mimicry case. Again we can draw conclusions about this only from comparable cases, as there is no direct information available. The facts found in those cases point to either of two possibilities in the case of parallel geographic variation of mimics and models: either the appearance of simple sex-controlled mutants which are simultaneously under control of the "switch genes"; or of an allelic mutation of these "switch genes" themselves. As long as no proof to the contrary is available, I prefer the latter simpler solution. We shall see below that the facts concerning intermediates corroborate such conclusions.

G. Parallel mutation

The foregoing discussion leads directly to another problem, which is more or less intertwined with it. We reported above Punnett's theory of the origin of mimetic resemblance by parallel mutation of identical genes. A comparison was made with the parallel color mutations in different species of rodents. Today, one could also allude to the examples in cultivated plants described by Vavilov (1922). Both Fisher and Ford (l.c.) take exception to Punnett's conclusion. They point out that the similarity between model and mimic is only superficial. Though the pattern may appear very much alike in general, this effect is produced by different means. (This statement is not developed in detail for the Lepidoptera in question. I assume that these authors meant that, for example, in one species, a spot in the cubital cell may have the same general pattern effect as a spot in a different cell in the other, etc.) The pigments involved may be chemically different. In addition, Ford emphasizes what he believes to be a corollary (impossible, as he thinks) to the idea of parallel mutations, namely, that the differences between dissimilar and often not closely related species ought themselves to be unifactorial if copied by a mimic exhibiting unifactorial mimetism. I seriously doubt the logic of this "corollary."

For a discussion of this topic we ought to distinguish clearly between different types of mimicry. If a *Kallima* mimics a dry leaf, parallel mutations in model and mimic are certainly excluded. If a beetle mimics an ant by a specific optical effect of shading on the thorax, again parallel mutation is excluded. If a Pierine butterfly mimics an Ithomiine, parallel mutation, however, cannot be exluded. If different elements of the respective patterns are able to mutate so that a similar effect is produced in both forms (see the example of industrial melanism) we may call this parallel mutation, as seen from the angle of the phenotype. If the yellow pigment N mutates to red and so does the chemically different pigment M, this is parallel mutation as far as the phenotype is concerned. I agree that Punnett did not make a good point of his case in detail. If a corollary to the idea of parallel mutation would be that both forms have "the same genes," the case of Fisher and Ford would be a good one. But if parallel mutation is—as the physiological geneticist has to contend—the presence of a definite developmental system which permits only phenotypical changes of a definite type, the situation is completely different. (Simile: The river Meander and the river Seine have a similar phenotype (meandering) because the pattern of flow can be changed only in some definite ways by orographical conditions. But the hills and rocks of Asia Minor and France may be basically completely different and thus might be the chemical composition of the respective waters.) Whatever the mutating genes, the phenotypical changes have their prescribed pattern. This deliberation shows that the facts mentioned by Fisher and Ford in no way preclude parallel mutation between two species of Lepidoptera (see below, under pattern). Thus in such cases as the parallelism of Ithomiines and Pierids, where nothing is known of their genetics, but where non-mimetic Pierids are known which show a potentiality for Ithomiine-like pattern in the Pierid system of wing differentiation, parallel mutation, as understood here, cannot be excluded *a priori*. But this problem does not enter such situations as those in which unifactorial inheritance is known in the polymorphic mimic.

Let us take first the case of one *Papilio* mimicking another, specifically the *memnon* case, with two non-mimetic females, one of which resembles the

male, and one mimetic female. According to Ford, few workers would be willing to accept the view that the model Pharmacophagus type *coon* has been derived from a *memnon*-like ancestor by a single mutation. We have mentioned already that there is no logical reason for this corollary. We do not know how the warning coloration of the model has been evolved; the fact (under dispute) that a similar type has been formed in a different species by a single mutation in no way gives information as to how the protected type originated. But I do not know of any reason why it should not have originated by a single mutation (we speak only of wing pattern). Actually, the pattern is not much different from that of other Papilios of the same general pattern group, certainly not more so than the patterns of the two non-mimetic females of *memnon* (which have not either been selected for pattern). Whatever the "genes" of the two *Papilio* species, the system of wing patterning available in the developing wings of the two species is such that changes in the same direction are possible, whatever the detailed procedure may be. This change may be brought about in one step in both species, or in one step here and many there, or in many steps in both, just as melanism can be one mutation or a set of additive ones. We might speak of a parallel mutation, in any case, but we must realize (what neither Punnett nor his opponents did) that parallelism does not mean the presence of "the same genes" or the same genetic history but of a similar developmental system which permits parallel changes to the same phenotype by many different genetic means. I might mention here, as I have on former occasions, the yellow mutant of the red-marked *Callimorpha dominula* and a geographic race of the same species in which the yellow is multifactorial. If both were to occur in the same locality, we would have a perfect example for the present discussion, except that both genetic types are here found in the same species.

The logical error in Ford's "corollary" becomes still more patent if we take the case of a mimetic female of a *Papilio* mimicking a Danaid or Acraeid. In my opinion, the unifactorial difference between one papilionid pattern and another mimetic one has no bearing whatsoever upon the genetic basis of the Danaine pattern. This might have originated in any conceivable way. It might be the ages-old pattern of the group, it might be the product of a slow or a fast evolution, or of direct evolution or one with whatsoever detour; all of which we do not know, as long as it is not possible to analyze this pattern by crossing experiments. But what has the origin of this pattern to do with the possibility that an existent pattern of a Papilionid may mutate into one of a type simulating that present in the Danaid? Clearly, then, the discussion of parallel mutation either does not make much sense, or else it amounts to a discussion of developmental potentialities, i.e., of the physiology of pattern formation. For illustration, I might return again to the model of melanism. We have discussed above the geographical "fashions" of South American butterflies of very different families, (Fig. 6) which included also parallel series of Heliconinae and Ithomiinae, a phenomenon which might or might not be connected with mimicry. Now it has been known that the same two groups may exhibit a parallel series of variations within the same locality (according to Kaye), which might be described as a kind of progressive melanism. One such case is illustrated in Fig. 7. But comparable series can also be found in other families and in another case described by Kaye, even in two species of the same family (Fig. 8). We can hardly err if we assume that, in the first case in both families and in the second in both species, the general features of the pattern system (namely, tendency to a barred pattern), whatever their genetic basis in either group, permit pattern changes which are necessarily parallel in both groups, because increase and union of dark bars will always look more or less alike. This shows clearly how we ought to look at so-called parallel mutation: not as geneticists but as embryologists.

We might, at this point, draw attention to a well known case of mimicry in plants, which Watkins mentions as a good parallel to animal mimicry and which makes a good case for properly understanding parallel mutation. The weed vetch, *Vicia sativa*, has been selected, clearly by the procedures of human agriculture, to resemble in many respects the lentil, *Ervum lens*, with which it is harvested, a case of parallel mutation, which could be discussed in the manner just applied to animal mimicry.

H. The intermediates and incompletes

We have described above the transitional forms found in different mimics, e.g., an intermediate form between the yellow and dark females of *P. turnus*, the intermediate females between two *Hypolimnas* forms and the different incompletes of

P. dardanus (see Table 4). In the *P. turnus-glaucus* case Ford speaks of incomplete dominance. But in the *Hypolimnas* example he insists that the re-

Fisher's theory requires. It seems that he draws the same conclusion from the existence of the incompletes in *P. dardanus*. He stresses the fact

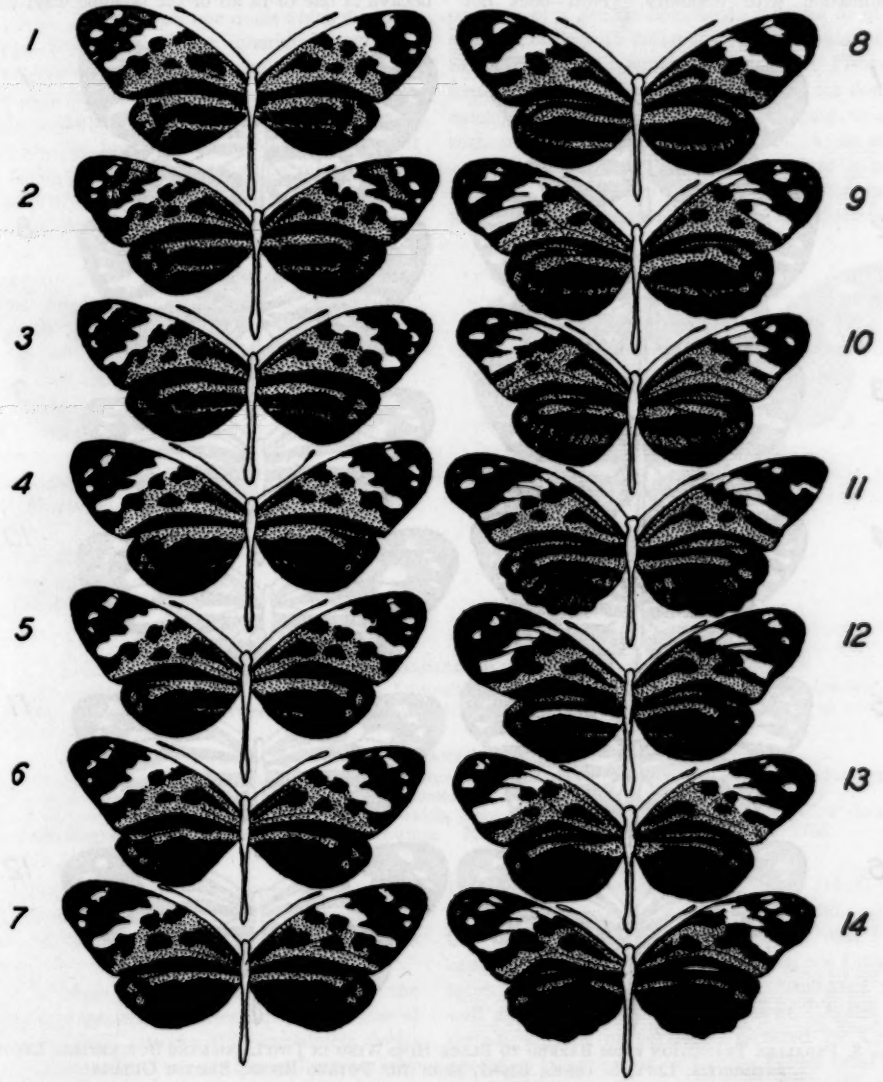

FIG. 7. PARALLEL TRANSITIONS FROM BARRED TO BLACK HIND WINGS IN AN ITHOMIINE BUTTERFLY MELINAEA MNEME (LEFT ROW) AND ITS HELICONINE MIMIC HELICONIUS NUMATA (RIGHT ROW), FROM THE POTARO RIVER, BRITISH GUIANA
Redrawn from photographs by Kaye

appearance of one intermediate together with two mimetic types in the offspring of an intermediate female (among eight females) is exactly what

that these are usually rare, but are very frequent in a region where the model is missing, i.e. where selection ceases. The implication is that in the

absence of selection for mimicry the modifiers of the "switch gene" may mutate, or recombine differently, or revert towards the ancestral combination with impunity. (Ford does not mention which of these genetic possibilities he prefers; maybe all.) Thus we must look at the available facts to see whether they are not only in general agreement with the theory, but are even exactly what is expected, as Ford claims.

Let us suppose that a system of the type demanded by Fisher's theory exists and that, with suspension of selection pressure, the modifiers behave in one or in all of the possible ways just outlined. Then the offspring of incompletes and intermediates ought to show a multiple factor segregation into many transitional types between the non-mimetic ancestral and the present form. Even with the numbers available in the families

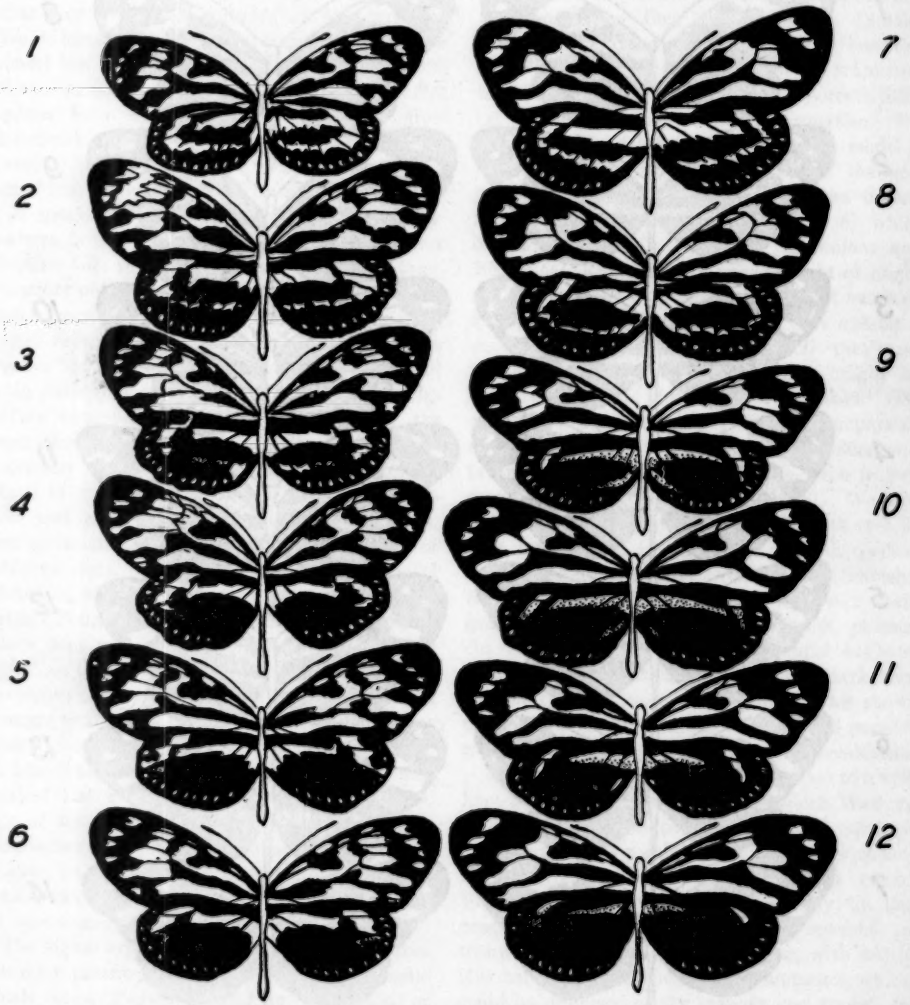

FIG. 8. PARALLEL TRANSITION FROM BARRED TO BLACK HIND WING IN TWO LYCOREANE BUTTERFLIES, LYCOREA PASINUNTIA, LEFT, L. CERES, RIGHT, FROM THE POTARO RIVER, BRITISH GUIANA
Redrawn from photographs by Kaye

studied by Ford something of this kind ought to become visible. But Ford himself points out repeatedly how simple the segregation is in the offspring of incompletes, in some cases clearly a 1:1 ratio. There is only one result which might be claimed for multiple factor inheritance, and this is not concerned with the relation of mimic to non-mimic but with a small variability of one feature (dark band) within one of the incompletes. Actually, the incompletes reproduce either their own type, or their own type and the mimic, or two different mimics, or even two mimics and both their incompletes, in what looks like simple dominance with a certain amount of plus and minus variation is not invoked, though no reason can be found for discarding this simple explanation. A change in dominance may be due to the presence of a genetic dominance modifier or of a multiple allele with different dominance relations. Selection for or against dominance in Fisher's sense may be involved or not; selection for dominance, however, does not have anything to do with selection for pattern modifiers. I do not see any reason why the *Hypolimnas* case is not simply a problem of dominance within the unifactorial determination of alternative patterns.

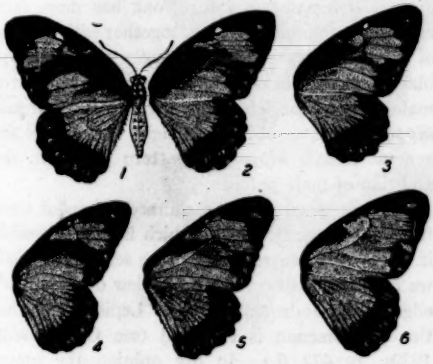

FIG. 9

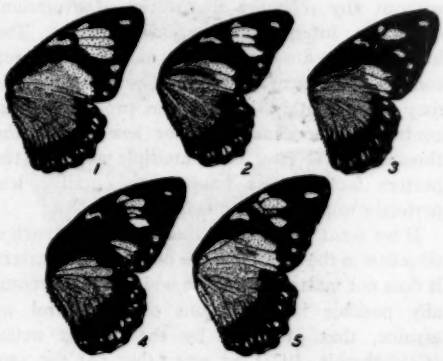

FIG. 10

FIG. 9. SERIES OF WINGS OF PAPILIO DARDANUS POLYTROPHUS BRED BY VAN SOMEREN

1 called *poultoni*, *4* called *proto-salaami*, *2* and *3* transitions towards *poultoni*, *5* transition to *dorippoides*, *6* *dorippoides*. Ground color brownish, yellow spots and patches of male yellow in ground. Redrawn from colored plate in Ford, 1936.

FIG. 10. P. POLYTROPHUS LAMBORNI (PROTOTROPHONIUS)

Variability of the intermediates; specimens partly captured (*5*) partly bred (the others). *1* is the type *lamborni*, *4* intermediate between *1* and *cenea*, others variants of *lamborni*.

Spots on both wings yellowish, ground color brownish, more or less small admixtures of male yellow (intermediate shading) are difficult to bring out in line drawings. Redrawn from colored pictures in Ford, 1936.

Mendelian ratios (although in many cases more than one mating is not excluded). I am unable to see how such results may be interpreted in Fisher's sense.

But is there any indication as to what the incompletes and intermediates are? In the case of *P. turnus*, Ford assumes that incomplete dominance is involved. The only reason I can see why this simple and obvious explanation is acceptable here, but not in the other cases, is that the difference between the two females of *turnus* appears to be small. In the *Hypolimnas* case, where intermediates between two mimetic females are found, the simple explanation of incomplete

In another case, studied by Carpenter (1913) in *Pseudacraea eurytus*, the genetics are unknown, but no reason exists for assuming a different explanation except that, if more than one female form is involved, modification of dominance as well as of epistasis might be present. Again, a certain amount of variation, pictured by Carpenter, is in agreement with other known cases of modification of dominance, e.g., for the *vg*/+ heterozygote in *Drosophila* (see Goldschmidt, 1935).

In the case of *P. dardanus polytrophus*, however, something different enters the picture. Here the male has the yellow (fluorescent) dark-banded

ancestral type with tails; the females are all mimics without tails and never have the basically different male pigment. The intermediates, however, have more or less of the male "ancestral" fluorescent pigment, more or less rudimentary tails, and more or less of the additional dark pigment which distinguishes the mimetic female from the male pattern. Such intermediates, and their range of variation, are pictured as well as it is possible without color, in Figs. 9 and 10 for the forms *protosalaami* and *prototrophonius*. In addition intermediates show a simple type of inheritance. If one looks upon such facts as a geneticist and especially as a physiological geneticist, i.e., without any reference to theories of evolution, only one interpretation seems possible. The intermediates are the product of incomplete sex control on a genetic basis. The genetic basis may be a mutant shifting the process of sex-controlled inheritance more or less beyond the threshold or it may be a multiple allele of the pattern factor which happens to entail a less perfectly working sex-control.

If we want to visualize the workings of such a situation in the determination of the sexual pattern it does not matter very much which of the eventually possible interpretations of sex-control we assume, that presented by the present writer (Goldschmidt, 1927), or any other not yet proposed. In a general way sex-control must always mean that the developmental patterning processes are proceeding in such a way that different thresholds for the action of the pattern factors exist in the two sexes, and this in an all or nothing set-up. Any environmental or genetic agency which affects this threshold condition may shift the sex-control partly or completely by disrupting the all or none reaction. (Excellent material for the demonstration of this viewpoint can be found in the behavior of sex-controlled characters in intersexuality, as discussed at many points in my books of 1927 and 1938. Actually one might call the *Papilio* intermediates wing-pattern-intersexes). It would be easy to develop specific ideas on this subject. But the main point is the general principle which, to me, seems obvious and in harmony with the facts.

A powerful argument in favor of our interpretation can be derived from a group of, I think, misrepresented facts recorded by Poulton. Already in 1906 (Plate 18) Poulton had pictured what he called a gynandromorph of *P. dardanus planemoides* with male spots on the left wings. Later he reported in detail (1927) on a group of eight gynandromorphs, which it is claimed were produced experimentally by dropping young pupae. I have discussed the facts already in my book on sex-intergrades (1929a) and I have shown that the interpretation must be wrong except for one specimen which is clearly a gynander, probably obtained by chance. Fig. 11 presents outline drawings of some of the specimens which, however, fail to show the male yellow pigment. Three of the others are *hippocoon* females with some splashes of male yellow color and more or less of tails. The genitals are female. Four are clearly males. One has a few patches of *hippocoon* female admixture; one has more such admixture on one hind wing, together with absence of the tail; another one is a male with scattered black female streaks on the fore wings; the fourth male has, on the left fore wing, scattered female *hippocoon* parts, while both right wings show the general female *hippocoon* pattern but with the addition of male patches.

Without discussing the claimed origin of these freaks by a tactile shock (which is not impossible if the shock occurred during the sensitive period) we may say with certainty, in view of our knowledge of gynandromorphism in Lepidoptera, that this phenomenon is excluded (see Goldschmidt, 1929a, p. 472 ff.). In my opinion the proper explanation is that in the males in question the mechanism of sex-controlled inheritance was somehow wrecked in part, so that here and there on the wing the dominant pattern of the other sex became visible. The females, however, should be explained exactly as has been done above for other "intermediate" females. This, then, seems to be the genetical situation for the incompletes. It is in no way opposed to Carpenter and Ford's explanation of the *distribution* of intermediates as a consequence of cessation of selective value for the mimetic pattern. But I believe I have shown that this selection has nothing whatsoever to do with the *origin* of mimetic patterns.

I. Pattern determination as basis of the problem

We have mentioned repeatedly that certain points under discussion required a solution from the standpoint of developmental physiology. The basic idea of the Fisher school is that it is impossible to imagine that the complicated pattern of mimetic resemblance could have been produced in one mutational step. It is clear that an objec-

tive analysis of this problem requires a knowledge of genetics and development of the wing pattern of Lepidoptera. Such information in regard to its actually known potentialities would permit conclusions concerning the possibility or impossibility of pattern changes of considerable magnitude. There is unfortunately no embryological (including postlarval development) and no experimental work on the present subject available for mimetic forms. But we know something about pattern development in other Papilionids (Goldschmidt,

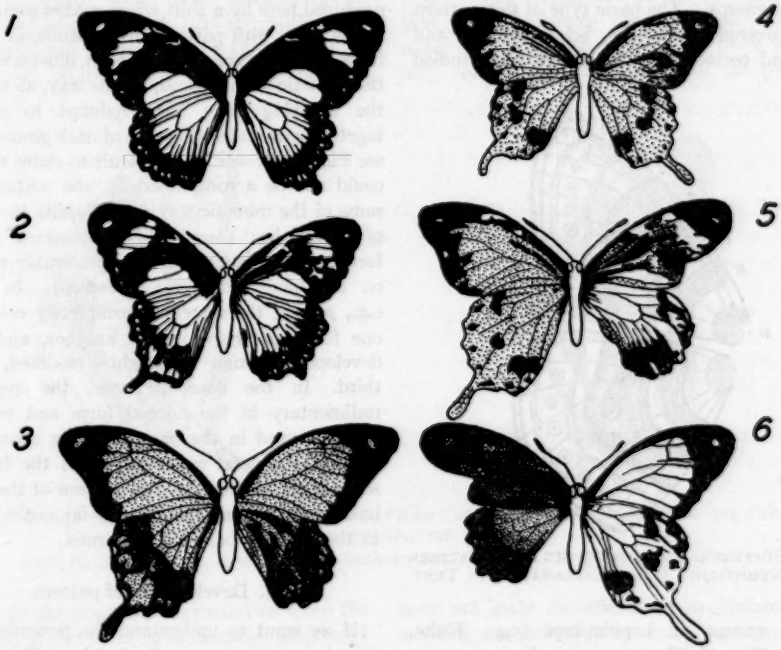

FIG. 11. A SERIES OF POULTON'S SO-CALLED GYNANDERS; ONLY *6* IS A REAL GYNANDER, LEFT ♀, RIGHT ♂
Notice more or less rudimentary tails in intermediates. Splashes of male yellow scales do not show in the drawing, after an uncolored photograph by Poulton.

1923, for *Thais polyxena*) and a good deal on other different aspects of the problem for a number of groups of butterflies and moths, including families which have furnished examples of mimicry (Nymphalines). A study of such facts, as far as they have a bearing on our present problem, will have to embrace the subjects: general rules of wing pattern, experimental shift within the pattern, potentiality of shifts without genetic change, the known types of genetic change, and the potentialities of change as derived from all other sources of information.

1. Comparative morphology of the wing pattern

A long time ago Eimer (1897), in his famous work on orthogenesis, tried to find rules governing the wing pattern of Lepidoptera. But only in recent times have Schwanwitsch (1924 ff.) and Süffert (1927 ff.) discovered independently what may be considered a general rule. There are a number of features in the patterns of both butterflies and moths which are of a more general nature: pigment may be deposited upon the wing veins and also upon the tracheae not enclosed in veins (extreme example—some tr opical Pierids); pigment may also be deposited in s tripes between the longitudinal veins (extreme example—some of the Asiatic Papilionids studied above); a ground pigment (also optical colors, e.g. *Morpho*) may cover the entire wing between whatever pattern is present (e.g., whites without much pattern, monarch butterfly with ground color between pattern); a ground pigment may cover only parts between the pattern elements, or more than one color may cover different cells or groups of cells either in both wings, or in only the fore or

hind wings (examples—the colorful admirals and underwings, and most tropical butterflies like the gorgeous Ornithopterae). All these general pattern types enter also the different mimetic patterns. In addition, the pigments in the patches may be chemically different, e.g., pierin, pteridin, different flavones; white may also be the optical effect of air-filled scales.

Aside from these generalized pattern elements there exists a special pattern of crossbands which basically is more or less alike in all groups, though it may be suppressed, enriched, or distorted in individual groups. The basic type of this pattern was discovered both by Schwanwitsch and Süffert and corroborated by others who studied

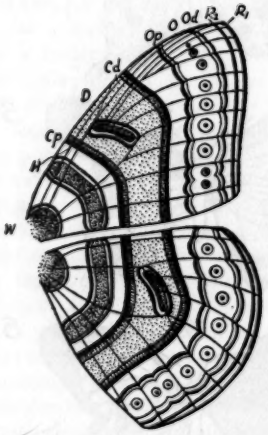

FIG. 12. SÜFFERT'S DIAGRAM OF THE BASIC PATTERN OF THE NYMPHALINE WING; EXPLANATION IN TEXT

different groups of Lepidoptera (e.g., Kühn, Henke). Fig. 12 illustrates this basic pattern, following Süffert's terminology. In the center is the central system of symmetrical bands Cd and Cp around the discoidal spot (D). Proximally from this the "hollow band" H is found, so-called because it frequently looks like a bowl with reenforced edges. Still more proximally follows the root band (W). H, W, and D tend to have a similar structure and are called the hollow elements. Distally there is a second symmetrical system, the ocellar system, with the symmetrical bands Op and Od with the ocelli in between. Two marginal bands, R_1 and R_2, follow the wing edge. It is remarkable how a great many different patterns may be derived from this basic one by splitting, fusion, disappearance, shortening, simplification or complication, and shifting of the main elements, processes in which the different systems (both symmetry systems, hollow elements, marginal bands) tend to vary independently. We need not go into details but point to the monograph of Henke (1936) in which this has been carried out in much detail for all important types of the Saturnid family. In the Papilionids, likewise, this sytem is the primitive one. It is still found in our *Papilio turnus* (which has a mimetic female *glaucus*, see Fig. 1). The actual pattern is produced here by a shift which makes some of the bands unite with parts of other bands, as Fig. 13 from another type of Papilionid illustrates. (In the mimetic form *glaucus*, by the way, all bands of the forewing have been reduced to shadows together with the appearance of dark ground color; see Fig. 1. It would be difficult to claim that this could not be accomplished by one mutant.) In some of the mimetic species of *Papilio* the system of bands has completely disappeared on the forewings and has also been considerably modified on the hind wings (e.g., *memnon*). In others, e.g., *polytes*, the system is completely covered in one form, partly visible in another, and better developed, though thoroughly modified, in the third. In the *dardanus* case, the system is rudimentary in the normal form and relatively little changed in the mimetic wings except for a change in general appearance and the fusion of some of the elements. The pattern of the models has exactly the same basis in so far as the relation to the primary bands is concerned.

2. Development of pattern

If we want to understand the potentialities of the wing pattern in regard to major shifts produced by a single causative agent, we must be acquainted with the development of pattern. Though the work on this topic is far from complete, we know at least some decisive points. These can be grouped in four classes, leaving out of discussion special features, such as relation to veins and sinuses. (A more detailed discussion and special literature may be found in my book, "Physiological Genetics".)

1. The location of the bands on the wing can already be demonstrated in undifferentiated wings. According to the school of Kühn, mitoses are frequent only along the path of later bands, i.e., a mitotic pattern is a pre-

cursor to the later visible pattern (see Kühn and Henke, 1929–1936).

2. At a later stage, but before any color appears, the pattern of the bands is completely differentiated. Goldschmidt (1920c, 1923) and students, e.g., Braun, found that the scales in the later dark bands of the pattern are soft and filled with blood at a time when those between the bands are already hard and finished. Thus a ghost pattern is produced which is based upon different speeds of differentiation in different sections of the pupal wing. The relation to the features of class one is that the mother cells of later dark scales in the bands have not yet finished multiplication and have not yet entered the phase of differentiation when the scales in between have already progressed considerably on the path of differentiation into scales.
3. In some cases (though not always) there is a relation between the shapes of scales and the pigment they receive (Kühn and students, see Kühn and Henke, 1929–1936).
4. The actual pigment does not appear simultaneously in the pattern but in a definite seriation. This applies to the pigment in the bands as well as to spots of a different color. (Work of Van Bemmelen, von Linden, a.o., literature in Biedermann, 1912.) Thus in one case studied by me (Goldschmidt, 1923) red spots on the hind wings of *Thais polyxena* were already pigmented while the rest of the wing still showed the ghost pattern; in addition the scales of these spots were completely finished.

Thus the main general features of pattern development consist in differential speed of growth and differentiation, and in a proper timing therewith of either the pigment-forming reactions, or the local availability of chromogens and oxidase, or the readiness of the chitin of the scales to imbibe these pigments. (This system can best be presented in curves of reactions and threshold conditions such as are to be found in my book of 1927.) This very general review already indicates that genetic changes (mutants) may act upon minor details if their effect takes place late, but may change the whole outlay of the pattern if they act early in differentiation (differentiation meaning the actual determining processes which usually occur before visible differentiation).

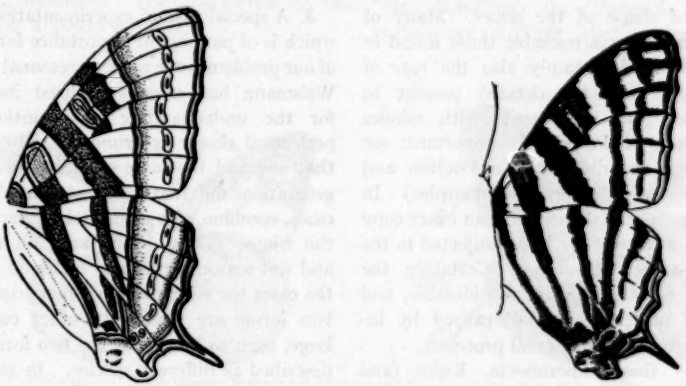

FIG. 13. WINGS OF CYRESSIS CAMILLUS, WITH A PATTERN COMPARABLE TO THAT OF PRIMITIVE PAPILIOS BUT EASIER TO ANALYZE (RIGHT)

Left, its interpretation in terms of the basic pattern elements. Redrawn after Süffert.

3. Experimental shift of pattern

The potentiality of pattern changes in a given wing can best be tested by non-genetic experimentation that uses simple and known agents. A very large body of facts has been collected, which may be summarized shortly under a few headings.

1. The pattern may be shifted by action of environmental changes upon the early undifferentiated wing. (The immense body of earlier work by Weismann, Standfuss, Merrifield, Fischer, von Linden, *et al.*, is minutely reviewed in Biedermann's monograph, 1912. For more recent work

by Federley, Kosminsky, Goldschmidt, and mainly by Süffert, and Kühn and his school, see the review in Goldschmidt, 1938a, and more recent papers by Kühn and Henke and Kühn and Engelhardt, 1936.) The classical temperature experiments with moderate heat or cold, and with heat or cold shocks, as well as experiments with CO_2, chloroform, etc., have shown that the imaginal wing pattern can be considerably changed by treatment of the pupal wing at a time before determination, the sensitive (or thermolabile) periods. The effects are: changes in ground-color, expansion or constriction of spots, union of spots and bands, disappearance of pattern elements or haziness of others, dissolution of eye spots into the constituent bands, disappearance of sexual dimorphism, and production of transparent wings with change of shape of the scales. Many of these individual features resemble those found in different species and certainly also the *type* of pattern changes (not the details) present in mimetic forms. (No experiments with mimics are known; they would be very important; see above, however, the discussion of Poulton and Van Someren's so-called gynandromorphs.) In some cases the experiment produces an exact copy of a geographical race of the form subjected to the shocks, a so-called phenocopy. Certainly the potentialities of patterning are considerable, and very different patterns may be realized by influencing the basic developmental processes.

In some of these experiments, Kühn (and students) found an additional important fact (see especially Kühn, 1926; Kühn and Engelhardt, 1936). The different pattern elements, e.g., the symmetry system, the marginal bands, etc., have different sensitive periods in which they can be influenced by environmental agencies. Thus, as a rule, the pattern does not react as a unit if the treatment is fractional, but will act as a unit if the treatment covers the whole period of sensitivity. In detail, the sensitive periods occur at different stages of development in different species (e.g., flour moth and *Abraxas*). They may be different for fore and hind wings, and also different reactions to cold and heat have been found.

Still more important are the experiments performed by Kühn and students on the flour moth, and by Henke on Saturnids (full review and literature in Goldschmidt, 1938a). They succeeded in inducing considerable shifts in the pattern by destroying (by heat) parts of the larval wing before the primary patterning processes take place. As the members of some of the systems of bands, e.g., the symmetry system, were found to be determined at the edges of a spreading stream of determination, an interference with this stream dislocates the bands correspondingly. The effect was a completely unusual pattern very different from the normal arrangement of bands. Generally speaking, this whole group of experiments indicates that very simple agencies, like blocking the path of patterning processes or shifting the time elements involved in these processes, may produce very different patterns in regard to small details such as size and color (and also structure, when optical colors are involved) of spots, or development of scales, as well as in regard to the primary pattern of the species, namely, the specific features of the bands and their derivatives.

3. A special case of experimentation on pattern which is of paramount importance for the analysis of our problem is the work on seasonal dimorphism. Weismann had already realized its importance for the understanding of evolution and had performed classic experiments. The basic fact is that one and the same species shows in different generations different wing patterns which, in some cases, combine with completely different shapes of the wings. Thus, spring and fall types or dry and wet season types may be found. In some of the cases the wing pattern differences between the two forms are small. In other cases they are large, even so large that the two forms have been described as different species. In such cases, the two types of pattern (and also shape) are quantitatively at least as different as the patterns of the polymorphic mimetic females of our present discussion. The only form experimented upon is that of the European *Araschnia levana-prorsa*, which is characterized by a rather considerable difference between the seasonal patterns. But the most extreme types have been described from tropical countries, such as *Prioneris watsonithestylis* from India, or *Precisoctavia natalensis* and *sesamus* from S. Africa. In Fig. 14 it is attempted to convey an idea of the differences in a black and white sketch. Temperature experiments (in *Araschnia*), initiated by Weismann, have shown that treatment of pupae in sensitive periods permits the production of all intermediates between the two patterns. (Good original photos of such a series in Goldschmidt, 1929b.) Süffert (1927) found later that one of the decisive features in determining the pattern differences is the presence or absence of a diapause. The details

do not belong here. The decisive fact for the discussion of our main problem is this: if a definite genetic situation is present in a species, a simple developmental mechanism involving a time element (retardation or speeding up of some reactions by temperature action or onset of diapause) can shift one pattern as a whole into a completely different one. In the case of proper experimental conditions the shift may be gradual, with all intermediate types. All this includes also some chemical differences (different colors), as well as shape of wings. Here, then, we have the visible differences of saltational magnitude, and also all grades of intermediates based upon one and the same genetical background but under different environment. Here certainly no room is left for combinations of modifiers. The application to the mimicry case appears obvious.

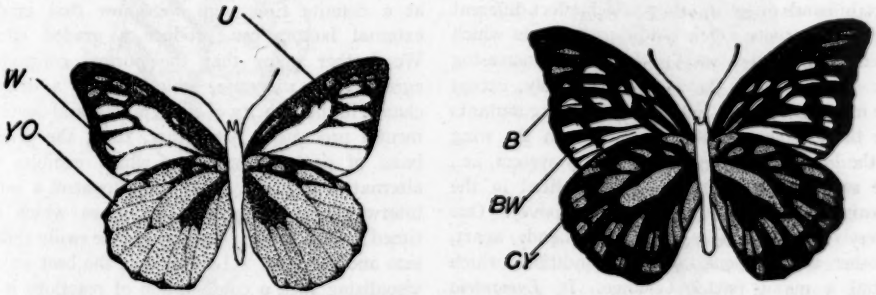

FIG. 14. THE TWO SEASONAL DIMORPHIC FORMS OF PRIONERIS WATSONI-THESTYLIS FROM INDIA (UNDERSIDE) AND OF PRECISOCTAVIA NATALENSIS-SESAMUS FROM AFRICA

Colors: U, umber; W, white; Yo, ochre; B, black; BW, bluish white; CY, chrome. Redrawn from a colored plate, after Doflein.

4. Genetics of pattern

We have demonstrated the existence of a potentiality for smaller or larger, localized or all-inclusive changes of wing pattern within the same genotype. These can be brought to realization by different but simple natural or experimental influences at the time of pattern formation. Now we must inquire whether simple genetic changes, i.e., one step mutations, can have similar effects. *A priori*, we must expect that all pattern changes which can be induced by environmental agencies should also occur as the effects of simple mutants. This conclusion is derived from the work of the present author (1929a, 1935) and his followers on phenocopies (see review in Goldschmidt, 1938a and 1940; a very detailed and careful repetition of my *Drosophila* work, with mostly identical results, has been published since by Henke and collaborators, 1941). The experiments show that the phenotypes of practically all known morphological mutants in *Drosophila* (and some in Lepidoptera) can be copied as modifications produced by experimental stimuli. Actually our knowledge of pattern mutants in Lepidoptera is not so complete as desired, one of the reasons being that only few groups have lent themselves to experimental breeding on a sufficiently large scale. A great part of the material is reviewed in Ford (1937). Most of the cases involve inheritance of such simple differences as ground color of the wing: red vs. yellow; both vs. white; black vs. light, i.e., the different groups of melanism, rutilism, leucism, etc. Sometimes the differences are unifactorial, sometimes multiple allelic, sometimes additive factors or modifiers are known; in addition autosomal, sex-linked or sex-controlled inheritance may be encountered. A number of these examples, in which elements other than simple color differences were involved, have been discussed above. Thus the phenotypic difference between *Abraxas grossulariata* and *A. lacticolor* (now called *dohrnii* König), the classic example of sex-linked inheritance, is not simply described as the paleness of *lacticolor*, but actually a reduction of all elements of the primary wing pattern, bands and spots, is visible. In the same form and in many others the reciprocal process, frequently included in the term melanism, is known, namely, expansion and confluence of the pattern elements, involving the entire pattern. In *Lymantria monacha* mutant alleles are known which produce such an effect

upon the pattern as a whole, while another mutant increases the pigment between the bands, i.e., the ground color.

Besides these generalized pattern mutants, which are usually included in the general classes of melanism and leucism (though some entomologists distinguish nigrism from melanism), a number of specific pattern mutants are known that influence definite single elements or a group of them (elements in the sense of the Süffert-Schwanwitsch scheme). The two species in which a relatively large number of such mutants are known are the flour moth *Ephestia kühniella* and the gypsy moth *Lymantria dispar*. In the flour moth a number of mutants have been found (by Kühn and students) which do not affect the pattern as a whole but affect the different pattern elements individually (pictures in Kühn and Henke, 1929, 1936). Thus mutants have been found which darken only certain bands or spots, others which affect different ones. But there are in addition mutants which affect the general coloring of the wing by increasing the breadth of all bands simultaneously, except the marginal spots. The most interesting mutants are those which affect the position on the wing of the bands of the central symmetry system, i.e., the same bands which could be shifted in the burning experiments (as reported above). One recessive mutant can pull these bands apart, another brings them together, conditions which entail a major pattern change. In *Lymantria dispar*, mutants are known (pictures in Goldschmidt, 1934) which affect the whole pattern in the same way as is typical for other melanistic varieties. In addition, mutants occur which affect definite parts of the pattern; one which changes the discoidal spots, or one which removes all bands except the marginal one. This is a rather large departure, as the zigzag bands are an important feature of the species. Still more extreme, in some ways, is a mutant which makes male wings almost white except for a dark rim, and simultaneously produces dark wing tips. This mutant is remarkable because it changes simultaneously the whole "costume" of the wing from one of uniform color, with the zigzag bands in the background, to one with a conspicuous pattern of differently shaded areas. These examples may suffice.

5. Application to the problem of mimicry

The facts of pattern determination in the Lepidopteran wing have been reviewed with the intention of proving that Punnett's theory of the origin of mimetic resemblance by saltation is in agreement with the known facts of pattern morphology, development, and genetics. The more general aspect of this conclusion can best be realized when we start from the phenomenon of seasonal dimorphism. Only few species of butterflies exhibit a considerable seasonal dimorphism with completely different patterns, though all Lepidoptera studied are liable to undergo more or less considerable pattern changes under experimental conditions. This means that the genetic background of the dimorphic species must, in a general way, control a type of development of wing pattern which is more easily shifted out of its typical sequence of events than is ordinarily the case. We do not have much knowledge of what this means in concrete terms. We know that the developmental processes can be affected only at a definite time, and we know that graded external factors can produce a graded effect. We further know that the normal controlling agent is the diapause, which entails a definite change in the velocity of all metabolic and developmental processes. Altogether, then, the genetic basis of pattern formation which exhibits the alternative form of reaction must control a set of interwoven developmental processes which are timed in such a way that they can be easily shifted into another gear. (I think that the best way of visualizing such a combination of reactions is by the diagrammatic systems of curves and threshold conditions which I presented in a simpler form in Goldschmidt, 1920a, and more elaborately in my book of 1927.) Whatever the details, the decisive fact remains that the potentialities for patterning have a very wide range which is contained in the genetic constitution, and that different agencies of a rather simple type can choose between realization of one or the other of the potencies just as we can make a definite tune be played by a "juke-box" by shifting one lever.

In my opinion, the origin of the mimetic pattern requires a similar genetic situation. The ancestral form must have had a type of pattern determination parallel to that present in seasonal dimorphic forms. This means a developmental system which can be switched into a very different one by one simple lever: in this case a mutation. The mutant must be such as to affect the *primary* patterning processes; it can change the pattern thoroughly because the proper developmental system is already available. Small wonder there-

fore that mimetism is about as rare as extreme seasonal dimorphism, and that it is confined to a few nearly related members of a few systematic groups (see above). I cannot see how such conclusions could be avoided in the face of the reported facts.

There is unfortunately no experimental evidence available of temperature, etc., action on mimetic forms and their non-mimetic relatives except the crude one of Van Someren discussed above. Such experiments performed on the non-mimetic (*meriones*), mimetic (*dardanus*), and variable races (*polytrophus*) of *Papilio dardanus* would be of supreme interest and might yield surprising results.

we know that these types are not different in principle from the different types of intermediates produced in seasonal dimorphic forms. The same holds for the different types of pattern produced by pattern intensification mutants in other forms. The bands which reappear under the influence of the mutant are obviously the ancestral bands of the group for which the potentiality is always present, just as the potentiality for wild type plumage is present in a white fowl. (The comparison is not meant to be genetic in detail.) The situation is simpler for the hind wings, in so far as the union of marginal bands, which is only a small step away from the non-mimetic form, constitutes a

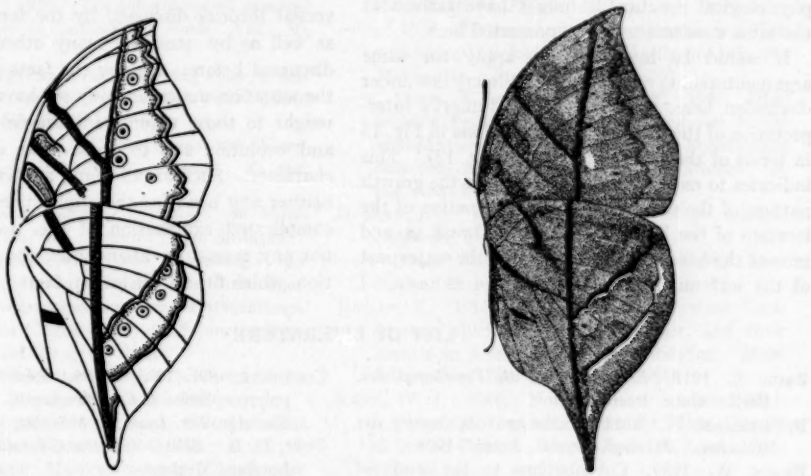

FIG. 15. WINGS OF THE LEAF BUTTERFLY KALLIMA INACHIS (RIGHT) WITH AN INTERPRETATION IN TERMS OF THE BASIC PATTERN (LEFT). Redrawn after Süffert

But some points may already be visualized by an inspection of the pattern of the mimetic and non-mimetic types (see Fig. 2). The non-mimetic forms show a large expanse of ground color with complete reduction of most bands (less so in the hind wings), but a union of the marginal bands into a broad margin. In all mimetic forms one main character of the fore wings is the presence of the anterior sections of the proximal bands and their gradual union with the margin, which leaves islands of ground color (with changes of pigment) as spots and patches in between. In the most extreme forms all of the wing, except a few islands, is covered with the dark pigment of the expanded bands. In some cases the bands near the root of the wing take part, partly or completely, in the process. From our previous knowledge of pattern

rather simple change (in addition to the change in color of the ground pigment). But in the hind wings (also in the fore wings in some forms) another pattern element frequently appears: the pigmented longitudinal bars in the center of the long wing cells between the veins. This is a pattern element for which the potentiality is generally present in the Papilio group. Actually it is the most prominent feature of the pattern of the Asiatic swallowtails. It is hardly necessary to continue to enumerate and to analyze other cases in detail. We think that it is clear that we are dealing with processes which an experimental embryologist might call the evocation of developmental potencies by the action of a mutant with the point of attack at the primary processes of pattern differentiation.

I might add that the foregoing analysis is, in many respects, a detailed elaboration of a short and general presentation of the subject in my book on the quantitative theory of heredity (1927). I find that an Australian entomologist, Nicholson (1927), has hinted at a nearly related viewpoint, not mentioning details of physiological genetics and development. He compared the pattern change in a mimetic pattern to the change of a picture when observed in blue light. A change of metabolism, he thought, might produce the whole pattern change, e.g., if the pigment is deposited earlier than usual. If he had been acquainted with the work on pattern formation and certain aspects of physiological genetics, he might have arrived at the same conclusions as are presented here.

It would be interesting to apply the same argumentation to other types of mimicry not under discussion here. I might submit Süffert's interpretation of the wing pattern of *Kallima* in Fig. 15 in terms of the basic pattern (see Fig. 12). This indicates to me that a single change in the growth pattern of the wing after the determination of the location of the bands will suffice to break up and reunite the bands in such a way that the major part of the leaf mimicry is accomplished at once. I might also point out that the same applies to any mutant whatsoever that influences early embryonic, i.e., patterning, processes. But this latter point has been sufficiently stressed in a recent book (Goldschmidt, 1940).

CONCLUSION

After reporting and discussing the significant facts, we came to the conclusion that Punnett's interpretation of mimetic polymorphism by mutation (saltation) agrees better with the facts than Fisher's neo-Darwinian theory. We have tried to prove this by scrutinizing in every detail both the logic and the factual basis of the controversial theories discussed by the former authors, as well as by studying many other aspects not discussed before. Among the facts pertaining to the situation under scrutiny we have given equal weight to those taken from the fields of genetics and evolution and to those of an embryological character. Such an all-round analysis has shown neither any need for the application of the more complicated explanation of the neo-Darwinists, nor any reason for abandoning the simpler solution, which fits all the known facts.

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THE EVOLUTION OF MALE HAPLOIDY

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INTRODUCTION

THE usual mode of sexual reproduction in Metazoa is that in which both sexes are produced from fertilized eggs. In certain groups of invertebrate animals, however, there occurs a type of reproduction in which the males are normally *impaternate* (fatherless), being developed from unfertilized eggs. These eggs have undergone reduction in chromosone number as if in preparation for fertilization, but since fertilization is lacking, the males developing from them have the reduced or haploid set of hereditary factors. The eggs will, if fertilized, normally develop into females. Hence the females are biparental in inheritance, the males uniparental. The Silesian bee breeder Johannes Dzierzon set forth this principle in 1845 with respect to the honey-bee, stating that drones, the males, come from unfertilized eggs, while workers and queens, the females, come from fertilized eggs.

The production of animals from fertilized eggs has been called *zygogenesis* in distinction to *parthenogenesis*, which refers to the development of an egg without fertilization. There are other types of parthenogenesis besides that described by Dzierzon. Unfertilized eggs may start as haploids and subsequently acquire the diploid number of chromosomes in some stage of cleavage, or they may start as diploids. The end result, in either case, is a fatherless (*impaternate*) but diploid animal. The evolution of such animals, interesting as it is, does not present the difficulties involved in explaining how the regular production of haploid adults has come about. In the case of impaternate diploids the chief problem is one of origin. In the case of impaternate haploids there are added, among other problems, those of survival with reduced chromosome number, of sex determination, and of spermatogenesis in the haploid male. Aside from the haploid males regularly produced in six or seven groups of invertebrates, there have been, with one or two exceptions, no known haploid adult animals. In contrast, diploid and polyploid impaternates are known to occur with more or less frequency and as independent events in many, if not most, of the larger groups of Metazoa. The origin of these will be discussed because of its possible bearing on the origin of male haploidy. Their method of sex determination is entirely orthodox, while that of haploid males involves a genetic mechanism essentially different.

Many of the hazards involved in the origin and survival of male haploidy which must have been overcome by evolution were stated by Schrader and Hughes-Schrader in 1931, in a discussion of the subject based on data available at that time. During the last decade, considerably more data have been accumulated, especially bearing upon sex determination in haploid animals, the problem which has been most difficult to explain, and it is for this reason that a review of the matter is now timely.

Different groups and species of animals combine different types of parthenogenesis with bisexual reproduction in their life cycles in various complicated ways. An excellent review of the subject has been given by Vandel (1931a), and no attempt will be made here to summarize what he has presented or to review more recent literature in detail. References given in this paper are selected, for the most part, from recent publications which show the extent and diversity of parthenogenesis, and thus have a bearing directly or indirectly upon the evolution of male haploidy, or in other words, the evolution of reproduction according to Dzierzon's Law.

Definitions

A considerable terminology has been developed in connection with the subject of parthenogenesis in which ambiguities are present, unfortunate, but certainly unavoidable in a science developed in

many lands and published in many languages. In order to orient the reader and to assist in expressing ideas more briefly, some definitions are given of terms to be used repeatedly.

The author follows Ray Lankester in using the term *impaternate* rather than parthenogenetic for the fatherless offspring developed from the unfertilized egg. Parthenogenesis may be *natural* (*spontaneous*) or it may be *artificial* (induced by some artificial stimulus). It may be *incomplete* (rudimentary), the embryo dying before maturity, or it may be complete, involving viability to the adult stage. It may be *obligatory*, occurring from a type of egg that cannot normally be fertilized, or it may be *facultative*, if the egg may develop whether fertilized or not. With reference to the sex of the impaternate offspring, parthenogenesis includes *arrhenotoky*, production of impaternate males, *thelytoky*, production of impaternate females, and *deuterotoky*, production of both sexes parthenogenetically.

The term *tychoparthenogenesis* (the prefix is derived from the Greek τύχη meaning chance, luck, or the favor of the gods) has been used for irregular cases occurring occasionally in zygogenetic species, either in nature or in experimental breeding (*Lymantria dispar* of Goldschmidt, 1917). The word may be extended to include a condition which may be regularly induced merely by preventing the female from mating (Tettigidae of Nabours, 1929, 1930) and which thus indicates a natural genetic tendency toward parthenogenesis. While the latter is, in a sense, induced tychoparthenogenesis, it is, of course, natural and facultative (spontaneous under the circumstances) rather than artificial (induced by artificial stimulus). With respect to sex of offspring, tychoparthenogenesis includes *tychothelytoky* and *tychodeuterotoky*. *Tychoarrhenotoky* has not been demonstrated. The possibility of its occurrence and the significance of the other two will be discussed in connection with sex determination.

Parthenogenesis may be *constant*, occurring in each successive generation, or it may be *cyclic*, in which case one or more parthenogenetic generations alternates with a bisexual. In cyclic parthenogenesis (*heterogony*) the parthenogenetic or agamic generation, the generation reproducing parthenogenetically, consists entirely or almost entirely, of females. The individuals of the bisexual generation, both males and (*gamic*) females, are impaternate. Parthenogenesis may occur as a *general* condition throughout the range of the species, or it may be *geographic*, in which case the parthenogenetic race occupies a different area from the bisexual. Males may be absent or very rare (*spanandry*) within the range of the parthenogenetic race.

With reference to meiotic phenomena in the egg, parthenogenesis may be *haploid* or *diploid*. In diploid parthenogenesis meiosis may be omitted altogether, there may be but a single oocyte division or, if both divisions occur, there may be fusion of two of the resulting haploid nuclei (*parthenogamy*). Diploid parthenogenesis may also be brought about by doubling of chromosome number in oogonial mitosis (*endomitosis*) to form the tetraploid number. Subsequent synapsis and two oocyte divisions give rise to a diploid egg which undergoes cleavage. The result, in any case, is a diploid individual which has been diploid from its beginning. Diploid parthenogensis may be said to result in a *zygote*, which in some cases is heterozygous, in others homozygous for genes that were heterozygous in the mother.

In triploid and polyploid parthenogentic races or species oogenesis takes place according to various methods as in diploid parthenogenesis.

In haploid parthenogenesis, meiotic phenomena in the egg are normal, with two oocyte divisions resulting in a haploid nucleus. The offspring is not, however, necessarily haploid, although starting as such, since chromosome doubling by endomitosis or by fusion of nuclei may occur during cleavage. Higher multiples of the haploid set are reported for some tissues than for others. Whatever the ultimate cytological conditions may be, the individual may be designated an *azygote* and, barring mutation, will breed true, all sperms or eggs produced by it being of identical genetic composition, since it originates from a haploid cell.

The term diploid parthenogenesis has often been used to include all cases in which the impaternate offspring have been judged on purely morphological grounds to be diploid, even though this diploidy results from "regulation" after meiotic reduction. This usage, developed before application of the genetic point of view, has caused much confusion and useless discussion, since chromosome number sometimes fluctuates in different tissues, and the criteria for haploidy are frequently very uncertain. In azygotes resulting from tychoparthenogenesis or from artificial parthenogenesis, this fluctuation is often very great. I believe that the extension of the term haploid parthenogenesis

to include all cases of origin from a single reduced maternal nucleus in animals should be helpful, by emphasizing a distinction of fundamental genetic significance between azygotes, whether haploid or not, and impaternate zygotes, which are diploid (or polyploid) in origin.

I have used the terms haploid or diploid as applied to azygotes just as I have used the terms diploid or polyploid for zygotes. These terms must be understood to be relative. Actually we do not know that any "diploid" animal is diploid in all of its tissues, and we do know that polyploid cells and tissues occur in many diploids. Similarly, we cannot say that any "haploid" animal resulting from haploid parthenogenesis is haploid throughout. The azygotes resulting from tychoparthenogenesis are, at least for the most part, diploid and possess the type of gametogenesis characteristic of normal diploid zygotes. The azygotes, males, produced by haploid arrhenotoky in the regularly established parthenogensis of Dzierzon's Law and the subject of discussion of this paper, are only more-or-less haploid, the drone bee being less, in fact apparently diploid with some polyploid tissues. Whatever the chromosome count may be in "male haploidy," meiosis is peculiarly modified or eliminated in the males so that spermatogenesis is of the haploid type.

While tychoparthenogenesis and the parthenogenesis involved in male haploidy are both facultative, they differ in that the former is poorly adapted, with many inviable cases of rudimentary parthenogenesis, while the latter is highly perfected, reproductively economical, facultative in the highest degree.

The parthenogenesis of male haploidy is to be contrasted with obligate arrhenotoky occurring in the male-producing females of the parthenogenetic generation preceding the bisexual in some of the groups with cyclic parthenogenesis. Other parthenogenetic females of these groups may be obligately thelytokous or deuterotokous.

In groups with constant parthenogenesis facultative arrhenotoky may be combined with deuterotoky. Fertilized eggs will then produce females, unfertilized will produce either males or females.

Many of the terms defined above are applied in various ways to individuals, fraternities, generations, races, species, or groups of higher taxonomic order, and this diversity of usage by different authors makes it difficult to clarify the subject. An individual of either sex may be either biparental or impaternate. A fraternity, generation, race, species, etc., may be either bisexual, consisting of males and females, or unisexual, made up of a single sex. The occurrence of induced parthenogenesis or of facultative parthenogenesis in a female, a race, a species, or a generation does not make it "parthenogenetic," for this term is used *sensu stricto* to indicate that the female or group is obligately parthenogenetic, that *all* offspring are impaternate. Thus a frog whose eggs are pricked and thereby stimulated to develop parthenogenetically does not become, through this process, different from any other frog, and is not said to be parthenogenetic. In the literature on artificial parthenogenesis the impaternate offspring are frequently designated as parthenogenetic, but this ambiguous usage is here avoided in accordance with the suggestion of Ray Lankester mentioned above.

A male should not be described as parthenogenetic, arrhenotokous, deuterotokous, thelytokous, or zygogenetic, but may belong to a race, a species, or a heterogonic generation characterized by any one of these terms. Exceptional or spanandric males occur in parthenogenetic (thelytokous) groups, but, except for these males, a parthenogenetic generation, race, or species consists of females only. Races or entire species are either parthenogenetic (female), arrhenotokous, deuterotokous, or zygogenetic. Parthenogenetic groups of taxonomic order higher than species do not occur, but such groups may be arrhenotokous, deuterotokous, or zygogenetic. A group (generation, race, species, etc.) reproducing according to Dzierzon's Law is said to be arrhenotokous. Thus the honey-bee *Apis mellifica* s an arrhenotokous, not a parthenogenetic or a zygogenetic, species. There are, however, races of the honey-bee that may produce females as well as males from unfertilized eggs. Such races are deuterotokous. The females of many species of grasshoppers are described as facultatively thelytokous because they produce females if they are unmated. The females of several species of moths are facultatively deuterotokous, producing impaternate offspring of both sexes. A certain parasitic wasp, *Nemeritis canescens*, although belonging to the arrhenotokous insect order Hymenoptera, is obligately thelytokous, a parthenogenetic species with males unknown. The European race of a certain stick insect, *Carausius morosus*, although belonging to the zygogenetic insect order Orthoptera, is obligately thelytokous,

a parthenogenetic race with rare, spanandric, males. The ancestral Asiatic type of the same species is bisexual and zygogenetic. This species therefore exhibits geographic parthenogenesis.

Use of the expressions arrhenotokous, deuterotokous, or thelytokous parthenogenesis is grammatically incorrect, redundant, because these adjectives imply parthenogenesis.

Extent of Occurrence of Male Haploidy

Production of males by haploid arrhenotoky is a stable mode of bisexual reproduction functioning quite as successfully as the more frequent method of zygogenesis of both sexes. It occurs in rotifers (Miller, 1931; Shull, 1929; Tauson, 1927; Wesenberg-Lund, 1923, 1930; Whitney, 1929), certain groups of mites (André, 1935; Cooper, 1937, 1939; Pätau, 1934, 1936; Putnam, 1939; Schrader, 1923), thrips (Davidson and Bald, 1931; Shull, 1917), white-flies (Morrill and Back, 1911; Schrader, 1920, 1926; Stoll and Shull, 1919; Thomsen, 1927), a certain tribe of scale-insects (Hughes-Schrader, 1927, 1930; Schrader and Hughes-Schrader, 1926), the beetle *Micromalthus* (Scott, 1936, 1938, 1941) and insects of the order Hymenoptera.

Most insects of this last-named order are much larger and more conspicuous than insects in the other orders and have, accordingly, received more attention from entomologists, cytologist, and students of heredity. For this reason, the Hymenoptera have contributed more than any other group to our understanding of the problems connected with male haploidy. Since reference will be made frequently to various members of the different subgroups of this order, they will now be very briefly characterized.

The order Hymenoptera is popularly said to include wasps, ants, and bees, but these divisions are by no means correlative. The great majority of species and major divisions are often called wasps. It is also often supposed that the order is characteristically made up of social insects. This is a misconception. Of the two large suborders, the Chalastogastra includes the more primitive species, plant feeders with caterpillar-like larvae, the horn-tails and saw-flies. Under the suborder Clistogastra, with maggot-like larvae, there are seven superfamilies. Four of these, Ichneumonoidea, Proctotrupoidea, Chalcidoidea and Evanioidea, include the vast majority of described species of the order, wasps which are largely parasitic on other insects and hence of great value economically. The gall-forming wasps and their relatives, Cynipoidea, make up the fifth superfamily. The members of the two remaining superfamilies, the aculeates or stinging Hymenoptera, are for the most part, wasps and solitary, as are all those previously mentioned. Of the fourteen families of the aculeate Vespoidea, only two show social development, the Formicidae or ants, which are all social, and the Vespidae or hornets, yellow jackets, etc., with some species solitary, some social in varying degrees, and with social organization polyphyletic within the group.

The seventh superfamily of the Clistogastra the aculeate Sphecoidea, includes three families of digger wasps, all solitary, and five families of bees, largely solitary but showing polyphyletic evolution of social life.

The entire order, irrespective of social organization, from lowest to highest in all of its divisions is characterized by parthenogenesis mixed with sexual reproduction (Peacock and Gresson, 1931; Sanderson, 1932; S. G. Smith, 1941). Biparental males (diploid) have not been reported from nature in any species, and meiotic phenomena in the male, as well as spermatogonial and somatic mitoses, indicate male haploidy. Dzierzon's Law applies as the rule. The sterile biparental males regularly occurring in the parasitic wasp *Habrobracon*, under conditions of inbreeding and "domestication" which will be discussed later, are not considered here. Oogenesis in all male-producing eggs is regular, with two oocyte divisions giving rise to the haploid nucleus. Naturally occurring exceptions to Dzierzon's Law are all in the direction of thelytoky, and in this case the oocyte divisions are variously modified. Even in the honey-bee, the species studied by Dzierzon and on which he based his principle, thelytokous races occur in which workers (females) produce workers without mating.

Many species in diverse groups are entirely thelytokous, females producing females indefinitely by diploid parthenogenesis with males entirely unknown (B. R. Speicher, 1937). In other species thelytoky is geographic, the female race occupying a newer region than the bisexual (arrhenotokous) (Brues, 1928; S. G. Smith, 1941). This condition is supposed to aid in the rapid spread of the species. Bisexual reproduction may be resumed later in the new territory.

In several gall-making wasps among the

Cynipoidea, alternation of a bisexual with an agamic female generation takes place (heterogony). The sexual females may be very different in appearance from the agamic, so that some have been described under generic names different from their own offspring. In the more highly evolved species of this group, a single agamic female produces either males or females but not both, and Doncaster (1916) has shown that from a single mating of a sexual female there are obtained only male-producers or only female-producers. Among the less specialized species a single agamic female may produce both males and females, and the tendency to produce one sex only appears to have evolved gradually within the group (Patterson, 1928a). In the agamic female generation parthenogenesis is obligatory, and the females, if introduced to males, refuse to mate. A few functionless males with weak mating instincts occur in this generation, probably from unfertilized eggs of the sexual females. They have been shown to be haploid, as are the normal males of the sexual generation (Patterson, 1928b).

In many species, among the minute egg parasites Chalcidoidea and Proctotrupoidea, polyembryony takes place, a single egg giving rise to more than one embryo, with numbers ranging from a few to many hundreds. Dzierzon's Law applies here, so that males only are produced if the egg is unfertilized, while a fertilized egg gives rise to a female brood. However, it has been shown that a few males, haploid, may sometimes appear in a female brood, indicating probable somatic reduction of chromosome number in the germ mass (Patterson and Hamlett, 1925).

Since parthenogenesis and male haploidy characterize the entire order from the relatively primitive horn-tails and saw-flies to the highly specialized parasites and the social wasps, ants, and bees, it must have appeared very early in evolution.

Taxonomically, the Hymenoptera are singularly isolated, with highly specialized wing venation even in the most primitive members, which show no relationship with any other orders. In the paleontological record there is also no evidence of relationship to other groups. The earliest forms occur in the Upper Jurassic strata and include both the plant-feeding Chalastogastra and primitive members of the parasitic groups in the Clistogastra,—Ichneumonoidea, Proctotrupoidea, and Evanioidea. Since several groups under both suborders are found in the Jurassic and since all known living Hymenoptera reproduce parthenogenetically, these extinct species must also have done so, unless we are to assume that parthenogenesis originated independently several times and spread through the entire order, which is highly improbable.

Fossil Hymenoptera are lacking in the Cretaceous period. The earliest aculeates date from the Eocene, at which time all the parasitic groups are also abundant. Among the ants, sexual females were distinct from the workers in the Tertiary, but polymorphism was absent among the latter, and differences separating the true workers from the soldiers were not evident until the Pleistocene.

PROBLEMS INVOLVED

Before an hypothesis is suggested to explain the steps in development of male haploidy, it will be necessary to consider certain problems concerning parthenogenesis, and some of the known facts which may throw light upon them. These problems may be discussed under five headings, as follows: (1) induction of parthenogenetic development in eggs; (2) viability of the azygote; (3) meiosis in the haploid male; (4) sex determination; (5) haploidy of the azygous male. The first two of these may be regarded as initial steps in the attainment of male haploidy. The remaining three problems will be considered before additional steps are presented.

Induction of Parthenogenetic Development in Eggs

Plants, with their normal alternation of an azygotic (gametophytic) with a zygotic (sporophytic) generation, have no block to post-meiotic mitosis. In animals this block is regularly established, with insurance thus added to syngamy but frequent lapses occur, initiating parthenogenesis. There is no need to discuss the causes of these lapses, which are the subject of innumerable experiments by physiologists and which fill volumes of research publications. Effective factors are chemical constituents of the water, temperature changes, and mechanical stimuli such as shaking or pricking the eggs, in addition to hereditary constitution. Lack of the block or its breakdown, as exemplified by the spore of a fern or the egg of a bee, is no greater problem than establishment of the block, as it occurs in the egg of a fly, for example.

In addition to experiments on artificial partheno-

genesis, the wide scattering of tychoparthenogenesis in different groups indicates the frequency of independent occurrence of this breakdown. Moreover, the presence of tychoparthenogenesis in some groups and in certain species, contrasted with its absence from others, demonstrates a racial or genetic basis.

Viability of the Azygotes

In presenting definitions concerning parthenogenesis, the distinction was made between haploid and diploid parthenogenesis, and it was pointed out that the former leads, not necessarily to a haploid animal, but often to a diploid azygote. In fact, tychoparthenogenesis is haploid, at least in the majority of cases, but in all cases the end result is a diploid. Most diploid zygous animals appear to carry recessive lethal genes which tend to be retained in the species because of their recessiveness. With the occasional development, in a normally zygogenetic species, of an unfertilized haploid egg such lethal genes disclose their presence, even though the egg develops into a diploid (homozygous) azygote. A high mortality of these azygotes (incomplete parthenogenesis) takes place, so that selection against recessive lethal genes must be very severe in tychoparthenogenesis. Many cases of incomplete parthenogenesis may be expected to occur in a zygogenetic species before an adult azygote appears. Having appeared, it is free from recessive lethals and ready to carry on should other conditions be conducive to that end.

The occurrence of such an azygote in the first place, together with its survival to maturity, indicates that it has certain genes required for successful parthenogenetic development. If it can reproduce, its offspring should, on the average, carry more genes tending toward parthenogenesis than the average of sexually produced individuals. Hence we might expect parthenogenetic races to appear by natural selection among the descendents of sporadically occurring diploid azygotes arising from haploid parthenogenesis, as well as of impaternates produced by diploid parthenogenesis in which recessive lethals would have no effect.

One definite condition which must be fulfilled (and which is undoubtedly accomplished by natural selection) if azygotes are to be viable, is a satisfactory surface-volume relationship in cells. Viable haploid sporophytes have been obtained in many species of plants without difficulty, whereas in normally diploid animals survival of a haploid is very infrequent. Fankhauser (1938) has been able by cold treatment to initiate haploid parthenogenesis in salamanders, but only one survived to a stage beyond sex differentiation. Surface-volume relationships in animal cells may be such that they place a much greater hazard on deviations from normal chromatin quantity than occurs in plants.

One method of regulation of the surface-volume relationship is by a doubling of chromosome number in the cleavage of an egg originally haploid, resulting in a restored but azygous diploid. Artificial parthenogenesis in frogs illustrates the problem of attainment of viability by chromosome doubling after parthenogenesis. Development is initiated by pricking the eggs (Kawamura, 1939; Parmenter, 1940). A small proportion of these gives off two polar bodies and undergoes cleavage. Embryos are highly inviable, but some have formed tadpoles, and a very few have metamorphosed. Of those not completing development, some are haploids, some diploids, and others are haplo-diploid or diplo-triploid mosaics, and even tetraploid tissue has been found. The increases in chromosome number come about before the first cleavage, during cleavage, or in later embryonic stages.

Because a large proportion of the impaternate tadpoles have been haploid and because others have had tissue in part haploid, it is certain that a majority, and perhaps all, were azygotes. The possibility is not excluded, however, that meiotic regulatory processes or parthenogamy from a polar body may have occurred in some, retaining maternal heterozygosis, and bringing about diploid parthenogenesis. It has not in all cases been noted that two polar bodies were extruded.

The cytological conditions of tychoparthenogenesis as it occurs in many invertebrates appear to be very similar to those in experimental parthenogenesis of frogs. There is a high mortality of developing embryos, and the survivors are diploid.

Before the remaining problems involved in attainment of male haploidy are discussed, certain types of diploidy in male Hymenoptera may be considered. This matter is taken up here because of its bearing on the viability of azygotes.

Chromosome number in the honey-bee has been a moot question for many decades. Nachtsheim (1913) summarized and discussed the various

views and was able to dispose of many of the cruder and less adequately supported hypotheses. His cytological study appears to be of the highest order. He regards sixteen as the haploid number of chromosomes and thirty-two as the diploid. Because there is reduction to eight in the primary oocyte, I prefer to regard eight as the haploid, and sixteen as the diploid chromosome number, and I consider his higher numbers as due to regulative splitting. Nachtsheim calls the first oocyte division reductional. By this he may mean either that the number of chromosomes is halved or that paternal elements are segregated from maternal. Presumably genes may segregate in either division, as in other forms.

In the following account I am disregarding Nachstsheim's terminology and his diagrams and am basing my statements on his excellent drawings of the actual material. Oogenesis is regular. There are sixteen chromosomes in the oogonia. Oocyte I metaphase is not shown. Eight dyads go to each pole in oocyte I anaphase. These dyads appear in both metaphase II spindles and divide into monads, eight of which pass to each pole in anaphase II.

Cleavage of the unfertilized male-producing egg shows that most of the chromosomes have already split in a regulative process, so that the number becomes sixteen, and this number is also seen in the blastoderm. It may be further increased in later stages. The chromosomes tend to lie in sister pairs because of their origin through regulative splitting.

Spermatogonia are not shown. There are sixteen dyads in the nucleus of the abortive spermatocyte I division. The spermatocyte II nucleus is divided equally, eight dyads going to each pole in anaphase. The chromatids may be separated so that they already appear as eight pairs of monads.

Cleavage in the fertilized egg shows thirty-two chromosomes, and approximately this number may be seen in the blastoderm. There is much discussion about the somatic chromosome numbers, some authors finding up to sixty-four (octoploid) in certain tissues of both sexes. It may well be that, due to regulation, the drone bee has, in general, the same chromosome numbers as the female, although it originates from a haploid egg. Nuclei of the male approximate in size those of the female (Oehninger, 1913; Nachtsheim, 1913). The fact that all the chromosomes are duplicated keeps the genic balance similar in all male tissues.

The number sixteen in spermatocyte I and in cleavage of the unfertilized egg is undoubtedly regulative. Reduction to eight in spermatocyte II is perhaps the last echo of ancestral male haploidy. This type of polyploidy, seen also in many tissues of other animals, may be called somatic polyploidy. It has no bearing on genetic ratios, so that the male bee should breed as a haploid, the female as a diploid. The number of linkage groups should be eight.

A second method of regulation for surface-volume relationships might be by increase in chromosome size. Size of nuclei and size of cells might thus be increased in the haploid male to approximate conditions in the female. Somatic haploidy has been demonstrated for certain tissues in certain species, but it is possible that regulation by doubling may take place in other tissues. Cleavage and gonial nuclei of males have been shown to have half as many chromosomes as those of females in several forms. This proves to be the case in the Ichneumonoid wasp *Habrobracon* in which wing cell size of "haploid" males approximates that of females (B. R. Speicher, 1935). The chromosome number in wing cells is, however, not known.

At present, many workers in this field of study take for granted haploidy of the males produced according to Dzierzon's Law, but considerable doubt has been expressed at various times in the past as to whether haploid organisms could be viable. In view of the fact that gametophytes of plants are normally haploid, this doubt was restricted to viability of haploid forms of organisms that are normally diploid. Subsequently, after the demonstration of occasional haploidy in sporophytes of plants, the doubt was still expressed as to whether animals might survive as haploids. Cytological studies indicating somatic polyploidy of the drone honey-bee seemed to strengthen skepticism as to the viability of haploid Metazoa.

In order to "explain" the viability of males in the Hymenoptera despite their presumed or apparent haploidy, and also in order to account for certain cytological phenomena, the suggestion has been advanced that the "haploid" set of the males is fundamentally diploid. Since females have been shown to possess twice the male chromosome number, in certain tissues at least, it follows that females would be tetraploid.

In several of the lower Hymenoptera there is pairing, to some extent, among the chromosomes of the haploid set in the male. Sanderson (1932) calls attention to this in the saw-fly *Pteronidea ribesii*, in which the eight haploid chromosomes appear as three pairs and two odd chromosomes, and Dodds (1938) shows secondary pairing of the ten metaphase chromosomes in spermatocyte II in the gall-wasp *Neuroterus baccarum*, in which the female may have five groups of four "homologues" each. Greenshields (1936a) discusses this situation, arguing that in the Hymenoptera there may be a tetraploid female-diploid male relationship instead of a diploid-haploid. No such pairing is evident in chromosomes of the Ichneumonoid *Habrobracon*, and Speicher (1936) points out that genetic data show *Habrobracon* to breed as a haploid. Greenshields (1936b) says that there is conflict in the use of terms by cytologists and geneticists, and that the genetic "facts are by no means fatal to a theory of derived tetraploidydiploidy based on cytological findings." He regards the phenomena of pairing as vestiges in the evolutionary sense.

In the evolution of cytogenetic systems, duplication plays an important rôle. The entire complex of chromosomes of a normal diploid form may be doubled, resulting in an *autotetraploid*. These show irregular types of pairing, multiple synaptic complexes, etc. Sterile species hybrids may double their chromosome number, resulting in fertile *allotetraploids*. These tend to have more regular meiotic phenomena than autotetraploids and are, consequently, more fertile. Duplication of single chromosomes or of parts of chromosomes may occur, thus increasing the number or size of these structures. Genetic duplication is often evidenced by irregular types of segregation, but this condition may later be masked by selective elimination of certain types, by further chromosomal rearrangement, and by genic mutation, so that "normal" conditions reappear.

Genetic polyploidy, or more generally, genetic duplication, must be regarded as relative. It is impossible to determine how much duplication or how much deletion may have occurred in the past history of the chromosome set normal to any species. In polyploid series in several groups of plants, the gametic number of chromosomes has been called haploid. This haploid number may be various multiples of a more fundamental *monoploid* number which, presumably, acted as the haploid number of some ancestral type. The term haploid is, therefore, a relative term. It is not improbable that there may be remnants of genetic polyploidy in the lower Hymenoptera or in certain higher groups, but this has disappeared or does not exist at all in others. In any case, the somatic regulative polyploidy of the bee appears to be a very different phenomenon.

Meiosis in the Haploid Male

In addition to cleavage of the unfertilized egg and survival of the azygote to maturity, the species with haploid arrhenotoky have attained some sort of regulation or omission of meiosis in spermatogenesis, so that the sperm cells have the full haploid set. Irregular distribution of chromosomes, such as occurs in haploid sporophytes (Bleier, 1933), has been avoided. This may involve a certain amount of telescoping of premeiotic processes with postmeiotic. If meiosis be deleted altogether, what appears to be a spermatogonial cell must enter directly upon spermiogenesis. The condition is here analogous to that obtaining in the formation of antherozoids in moss or fern gametophytes, in which there is, of course, no heritage of meiosis.

Among the mites this condition is approximated. Schrader (1923) indicates no distinction in *Tetranychus bimaculatus* between spermatogonia and spermatids, each with three chromosomes, the haploid number. There is no growth stage, and meiosis is omitted. Pätau (1934, 1936), showing that the male of *Pediculoides ventricosus* is haploid with three chromosomes, did not follow spermatogenesis because he considered the cells too small. Presumably there is also no appreciable growth stage in the haploid "spermatocytes" of this mite.

In the homopteran insects of the family Aleurodidae or white-flies, the males are haploid. Schrader (1920) states that in the males of *Trialeurodes vaporariorum*, with eleven chromosomes, the "reduction" division is omitted, and the "equation" division is not differentiated from the last spermatogonial. Thomsen (1927), however, states that in *Aleurodes proletella* the spermatocytes have a clear growth stage, but there is only one meiotic division.

Most haploid males make some compromise with meiosis. This is effected in various ways in different groups.

In the one beetle shown to have haploid arrhenotoky, *Micromalthus debilis*, Scott (1936)

studied spermatogenesis. The ten chromosomes of the abortive first spermatocyte anaphase move away from the single pole, with the attachment fibres lengthening. The second division is normal giving rise to two normal spermatids.

In the rotifer, *Asplanchna amphora*, Whitney (1929) showed thirteen chromosomes dividing in the first spermatocyte. Two types of sperms were found. The larger motile functional type, with thirteen chromosomes, the haploid number, develops directly from the secondary spermatocytes. A few of the secondary spermatocytes, however, divide with irregular distribution of chromosomes, to form spermatids with a small amount of chromatin. These develop into small non-functional spermatozoa. Tauson (1927) found but a single spermatocyte division with the reduced number twelve in *Asplanchna intermedia*, the second division being lacking. In this species she failed to find any rudimentary spermatozoa.

In the scale insects, Coccidae, of the tribe Iceryini, the males are haploid with two chromosomes. There is a single meiotic division of the first spermatocyte nucleus. The cytoplasmic division is suppressed and binucleate spermatids are thus produced from each of which two sperms develop (Hughes-Schrader, 1930). In the cottony cushion scale, *Icerya purchasi*, the females are secondarily modified into hermaphrodites. These have four chromosomes (Hughes-Schrader, 1927). The males, which are infrequent, are similar in spermatogenesis to males of the related species having females. Spermatogenesis of the haploid cells in the hermaphrodite gonad tallies in every respect with that in the males. Diploid cells in the hermaphrodite have also shown spermatogenesis in limited regions. It is clear that two divisions occur and that each sperm receives the haploid number. In several cysts of large cells there were four chromatin threads in various phases of condensation. Early telophases were found with four rods at each pole, and a few binucleate cells with four chromosomes in each nucleus. In the second anaphase two chromosomes go to each pole. As in the haploid spermatocytes of the hermaphrodites and of the males, there is no cytoplasmic division. The secondary spermatocytes from the diploid cells are therefore binucleate, with four chromosomes in each nucleus, and the spermatids are quadrinucleate, with two chromosomes in each nucleus. Each spermatid from the haploid spermatocytes develops two sperms and each spermatid from the diploid spermatocytes develops four sperms. In this form, apparently, normal meiosis with reduction can take place in spermatogenesis in diploid tissue.

With the exception of *Telenomus*, in which it is the second spermatocyte division that is abortive (Dreyfus and Breuer, 1944) the reported cases in the Hymenoptera show failure of nuclear division in the first spermatocyte, which usually gives off a small cytoplasmic bud. The second division is equal, and two sperm cells result, except that in the bees there is but one sperm cell which develops from the larger of the two very unequal spermatids. In the different hymenopterous species studied, details differ, as in degree of condensation of chromosomes during the first "division," in which also the cytoplasmic bud may be lacking.

That certain types of meiotic regulation become fixed in the heritage of different groups is shown also in the spermatogenesis of diploid males in *Habrobracon*. Here there is asynapsis; a cytoplasmic bud is given off, as in haploid males, and an equational division of the entire set of twenty chromosomes ensues, giving rise to two diploid sperms (Torvik-Greb, 1935). These may occasionally function in the production of triploid females. It is of interest to contrast the failure of reduction in the spermatogenesis of the diploid males of *Habrobracon* with its presence in the diploid spermatogenesis of the hermaphrodite *Icerya purchasi*. Possibly the general pattern of spermatogenesis in the Hymenoptera is more definitely fixed genetically, or it may be that since the hermaphrodite coccid is but little removed from the female, its diploid spermatogenesis takes on something of the character of oogenesis.

Regulation of meiosis in haploid males has, as we have just seen, been accomplished in a variety of ways. One or another of these methods must have been selected in the attainment of male haploidy from an assortment of diverse conditions depending in part, at least, upon the normal mode of the ancestral diploid stock.

Sex Determination

Zygogenetic back-cross sex determination

With the development of the Mendelian principles of heredity, it has been shown that sex determination, as a rule, is comparable to the "back-cross" of the heterozygous dominant to the recessive. Two general categories may be recog-

nized,—the XY-male type and the WZ-female type.

Most animals and many dioecious plants belong to the XY-male type. In these one-half of the sperm cells receives the recessive factor X, the other half receives the dominant factor Y. All eggs receive the factor X from the homozygous recessive female, XX, in which crossing-over takes place. Cytological conditions corresponding to these factors are very diverse, especially in the male. The entire X-chromosome may segregate as a unit from the Y in meiosis, as in the fly *Drosophila*, or there may be crossing-over between portions of the X- and Y-chromosomes, as in man and in some fishes. In the latter case the factors X and Y correspond, not with entire chromosomes, but with those parts only that fail to cross over. The Y-chromosome may be larger than the X, it may be the same size and indistinguishable cytologically, or it may be smaller. There may be several small Y-chromosomes which segregate as a group from the X, or there may be no Y-chromosome. The factor Y then corresponds to the small chromosomes or to the absence of the X-chromosome.

The WZ-female type of sex determination is found in birds, in moths, and in some fishes. What has been said about the chromosomes in the XY-male type may be applied here also, but just in reverse as regards the sexes. The male has two Z-factors and is, therefore, recessive in a Mendelian sense. The female is heterozygous dominant, with W dominant over Z.

The sex of an individual is determined by the ratio existing between male-producing and female-producing genes. These sex genes may occur in the autosomes as well as in the sex chromosomes. They may be absent from one or the other of the sex chromosomes. In *Drosophila*, with sex genes absent from the Y, and X is predominantly female-producing and the autosomes predominantly male-producing. In the dioecious plant *Melandrium*, the Y is highly male-potent in genic content, the X is female-potent and very little if any sex potency has been located in the autosomes (Warmke and Blakeslee, 1940).

A single set of autosomes is designated A. In XY-male sex determination, the complete male formula is XY + 2A, the female formula 2X + 2A. However the sex genes may be arranged, there are more female-producing genes in the female, more male-producing genes in the male, and the difference in the ratio determines the sex. Since the autosomes are normally the same in the two sexes, they may be left out of the formula. Sex differentiation depends upon the type of sperm cell which fertilizes the egg.

In WZ-female sex determination, the male formula is 2Z + 2A, the female formula WZ + 2A, and the same principles outlined above apply, but in reverse as to the sexes.

"Protection" from natural selection exists in the heterozygote, so that one chromosome may degenerate, losing most of its genes, as has the Y in *Drosophila*, or becoming small in size or disappearing. Muller (1918) expressed this idea, stating that lethal changes and deleterious mutations as well as visible recessives "should be found to have arisen in any chromosome region that has been protected for a long time from the action of natural selection. . . . The Y chromosome is so protected by the X."

Geneticists often use the formulae of the cytologists, distinguishing XO males from XY males. These two formulae are, of course, inadequate to express the great variety of cytological conditions that exist in the XY-male type. Since it is usually unnecessary to express these differences in genetic formulae, I am, for the sake of simplicity, proposing to use the terms X- and W-factors as female differentiators, Y- and Z-factors as male differentiators. Linkage and crossing-over may then be expressed with the alleles X vs. Y and W vs. Z.

Chromosome constitution and, therefore, the sex of the individual, is usually determined at fertilization of the egg, but this may occur at other times, as, for example, during cleavage in certain zygogenetic animals. In the fly *Sciara* all zygotes begin as 3X + 2A but subsequent elimination of X's occurs so that the soma of the male becomes X + 2A and that of the female 2X + 2A (Metz, 1938). *Sciara* therefore belongs to the XY-male category.

In parthenogenesis, the chromosome constitution of the offspring is usually decided during meiosis. In the agamic generations of aphids, both X-chromosomes are, in general, retained in the egg so that females, 2X + 2A, are produced. However, one X-chromosome is regularly extruded from the male-producing parthenogenetic egg, leaving X + 2A.

Tychoparthenogenesis is usually haploid, with restoration of the diploid number by endomitotic doubling. The offspring, starting as haploids, are therefore homozygous both for sex and for other factors. With XY-male sex determination the

offspring are, in general, females, 2X + 2A, from the X + A eggs laid by the 2X + 2A mothers. The rare exceptional males occurring here may be explained by failure of the X to double at the first cleavage. With WZ-female sex determination the offspring are males, 2Z + 2A from the Z + A eggs, and females, 2W + 2A from W + A eggs, in equal numbers. The exceptional composition of the females, WW instead of the usual WZ, will be discussed later.

Origin of parthenogenetic races and species

Parthenogenesis, as it exists in thelytokous races, rarely if ever is haploid. Parthenogamy from a polar body may occur as in *Lecanium* (Thomsen, 1929), but the more usual condition appears to be an advance of cleavage into meiosis so that an oocyte nucleus becomes the first cleavage nucleus. This advance may result in omission of synapsis, so that the nuclei resulting from the last oogonial division enter immediately upon cleavage. The diploid (or polyploid) group may then accumulate by mutation a considerable amount of heterozygosis. Obvious variation will then be limited to dominant changes, and different lines, lacking the possibility of crossing (amphimixis), will differ in many unseen (recessive) traits.

Vandel (1931a, 1936, 1937, 1940) regards tychoparthenogenesis (haploid) as the primitive type from which there has developed the normal and regular (diploid or polyploid) parthenogenesis of thelytokous races. Doubling of chromosome numbers, which occurs somatically in tychoparthenogenesis, is extended in thelytokous races to involve the germ track. Vandel (1936) makes comparisons in nuclear phenomena between tychoparthenogenesis, which he calls genotypic, and experimental parthenogenesis which is phenotypic, initiated by artificial means. Either type may be incomplete (rudimentary) or may result in an animal viable to maturity. He traces the origin of polyploidy in parthenogenetic races to a persistence and an extension of endomitosis or parthenogamy. He omits discussion of male haploidy, which he regards as a specialized problem insoluble from the data available to him.

In the evolution from the usual bisexual zygogenetic type of reproduction to the thelytoky of parthenogenetic races, there is elimination of the male and a tendency toward polyploidy. In the evolution of male haploidy, progression is in the reverse direction. Parthenogenesis produces males with reduced chromosomes. Since the race cannot consist entirely of males, a limit tends to be set to selection for this reduction, because the biparental females are necessarily diploid. There must also be established some genetic mechanism effecting physiological harmony of haploidy and diploidy in the same species. An answer to this problem is suggested in the nature of the sex differentiating factor which will be discussed below.

Thelytoky

Parthenogenetic races or species are, of course, female, thelytokous, except for the rare or spanandric males. The type of parthenogenesis is diploid (or polyploid), and the chromosome complement of the mother is passed on unchanged to the daughters.

Induced parthenogenesis may be either diploid or haploid. In the latter case mortality is very high, and the diploid number of chromosomes is usually restored in the few surviving individuals.

Let us first review parthenogenesis in some of the groups which are known to be of the XY-male type or are probably so. Here females are to be expected.

One impaternate rabbit, normal and presumably diploid, from induced parthenogenesis was raised to maturity by Pincus and Shapiro (1940), a female as expected from an XX mother.

Fankhauser's (1938) *Triton* from induced parthenogenesis, the only amphibian attaining the stage of sexual differentiation as a haploid, was female. It is not known whether salamanders are of the XY-male type.

The few impaternate frogs from experimental parthenogenesis reared to the stage of sexual differentiation were all diploid. They included individuals of both sexes and also a number of sex-intergrade types with considerable asymmetry. The frog is presumed to belong to the XY-male category, although there is no convincing cytological evidence, and genetic data are derived from race and species crosses and are not clear-cut. Kawamura (1939) who obtained only five frogs with testes among twenty-three impaternates, concludes that "the preponderance of females and the absence of typical males can be explained well by the theory of male digametism." The parthenogenetic origin of these frogs, with irregularities in chromosome regulation, may account for their tendency toward hermaphroditism, which

was to some extent effective although they belonged to the sexually "differentiated" race.

Parthenogenesis is very rare in the two-winged flies, the Diptera. A few parthenogenetic races and species have been reported, especially in the Chironomidae (Sturtevant, 1923; Thienemann and Strenzke, 1940a, 1940b). Nothing is known of their cytology. It is of interest to note that paedogenesis, a type of parthenogenetic reproduction by larvae, also occurs in this same family. In haplo-diploid mosaics of *Drosophila*, Bridges (1930) showed the haploid region as well as the diploid to be female, as was to be expected from the known fact that Diptera are XY-male in type.

Parthenogenetic species have been recently reported in the Embioptera (Ross, 1940), the first parthenogenesis for this small order of neuropteroid insects, and in the Psocoptera (Badonnel, 1938), the "book-louse" order. Both species are known only as females.

Induced tychothelytoky has been shown in crickets (Ohmachi, 1929) and in termites (Light, 1944). Crickets, and probably also termites, the socialized relatives of cockroaches, are of the XY-male type.

Phasmids are of the XY-male type. There is an extensive literature from field studies and from breeding experiments showing geographic and constant thelytoky in many species (Cappe de Baillon and de Vichet, 1939, 1940). It is also certain that tychothelytoky occurs.

In the Saltatoria (jumping Orthoptera), parthenogenetic species appear to be rare. In the Tettigoniidae, Cappe de Baillon (1939) and Matthey (1941) have reported parthenogenetic species, with polyploidy probable from comparison of chromosome numbers in related forms.

Induced tychothelytoky has been shown in four species of the grouse locusts, Tetrigidae, in the extensive genetic experiments carried out by Nabours (1929, 1930). In one of these, *Apotettix eurycephalus*, among more than 5,000 impaternates bred to maturity, only thirteen were males. All were homozygous for numerous traits for which their mothers were heterozygous, indicating haploid parthenogenesis (King and Slipher, 1933). The homologous (sister) chromosomes in these impaternates tend to be approximated in the metaphase plates of cleavage in unfertilized eggs, while in fertilized eggs the homologues (members of paternal and maternal pairs) are widely separated (Robertson, 1930). Induced tychothelytoky also occurs in several Acrididae (Slifer and King, 1932; King and Slifer, 1934; Creighton, 1938). The meiotic process is normal, being unaffected by the absence of the sperm. The haploid set of early cleavage is doubled as development progresses, so that some, perhaps all, cells of the embryo become diploid. Development is successfully completed in few of the individuals, and these are females. The rare impaternate males in Nabours' grouse locusts may be readily accounted for by accidental loss of an X-chromosome or its failure to double along with the autosomes.

Insects of the order Lepidoptera, moths and butterflies, possess the WZ-female type of sex determination. Among moths of the family Psychidae and of the Lymantriid genus *Orgyia*, there are parthenogenetic species and geographic races which may be diploid or polyploid (Seiler and Schaeffer, 1941).

Astaurov (1940) has induced thelytoky by heat treatment in the silk-worm, *Bombyx mori*. The unfertilized eggs were immersed in water at 46.0° C. for eighteen minutes. About 500,000 caterpillars capable of transforming to adults were produced over the period from 1932–1937. Of 37,152 identified as to sex, 37,139 were females and only thirteen males. The great majority were of maternal phenotype and genotype. Cytological studies by Frolova showed that the eggs undergo a single division, presumably equational, and that the diploid number of chromosomes is retained. Eggs of impaternate females were subjected to treatment each generation for six successive generations. Tetraploid females also appeared, and these were perpetuated parthenogenetically by heat treatment. When tetraploid females were crossed with normal diploid males, there resulted sterile triploid males and females.

Deuterotoky

Several instances of tychodeuterotoky have been reported in different species of moths. The literature has been reviewed by Vandel (1931a) for these examples of infrequent facultative parthenogenesis, in which males and females are produced in approximately equal numbers.

T. L. Smith (1938), carrying out genetic experiments with x-ray mutant types in the wax moth *Galleria mellonella*, showed that while mated females lay from two hundred to one thousand

eggs, which hatch in ten days, about ten per cent of unmated females show facultative parthenogenesis, and hatching requires several weeks. The impaternate offspring from a single female number from one to ten only, but these are normal in every way. Only seventy-nine attained an age in which sex was identified. There were forty males and thirty-nine females. When the mothers were heterozygous for recessive autosomal traits, the impaternate offspring were nineteen wild type and nine recessive.

According to genetic evidence, Lepidoptera have the WZ-female type of sex determination. Cytologically, females may be ZO or ZW. In the moth *Phragmatobia* there is one large Z- which segregates against two small W-chromosomes. Many species have no visibly differentiated sex chromosomes, and here it is presumed that the W-chromosome is equal in size to the Z. Chromosome numbers tend to be rather high in the Lepidoptera, and there is probably a considerable amount of distant phyletic polyploidy, in addition to the more recent cases in closely related species or varieties, in which one has double the number present in the other.

Explanation of the 1:1 sex ratio obtaining in the tychodeuterotoky of Lepidoptera apparently lies in the fact that it is haploid with subsequent doubling of the chromosome complex, and that the WW females, as well as the ZZ males, are viable (Seiler, 1923). It might have been expected that WW, which may be a homozygous deficiency, would be inviable and that all-male broods would result. Such have not been found, however, and we must attribute the viability of the females to vestigial polyploidy, with Z-factors duplicated and acting as autosomes (females Z'Z' ZO, males Z'Z' ZZ).

Sex determination in male haploidy

In all types of parthenogenesis other than male haploidy the principle of genic balance applies, so that sex determination *sensu stricto* is orthodox, however irregular may be the determination of chromosome distribution. This is true because the ratio of male-producing to female-producing genes is higher in males than in females, just as it is in "back-cross" sexual reproduction.

In haploid arrhenotoky, however, as it exists in the Hymenoptera and in the other groups mentioned above, sex determination has appeared to be fundamentally irreconcilable with generally accepted principles. This has been clearly stated by Bridges (1925). "To me, sex determination in the bee is the outstanding unsolved puzzle, although before the development of the idea of genic balance it seemed one of the clearest and simplest of cases. If it is true that the male is a haploid individual, then we would suppose that the diploid individual should likewise be a male, since the ratios among the sex-determining genes are not different in the two cases."

Three hypotheses were suggested to meet this difficulty before adequate genetic experiments had been carried out. One resorted to the cytoplasm, haven of cytogenetic theoreticians unable to make a better known port. The cytoplasm was supposed to transmit indefinitely a male tendency strong enough to dominate a single set of female-producing chromosomes, but dominated by the diploid set. This hypothesis disregarded the fact of regulation, assuming that the ratio of genic material to cytoplasm in azygotes was only half that in zygotes.

The second hypothesis placed sex differences in the Hymenoptera in the same category as those noted in different chromosomal types with multiples of the single complete set, in so-called euploid series. Such differences are seen in the rougher texture of eyes and larger size of triploid females of *Drosophila* and in the broader leaves of polyploid plants. However, the complicated sex differences found in insects are obviously of a very different order from these. This theory was elaborated into an algebraic sum hypothesis for male-determining and female-determining genes. When applies to the various sex and chromosomal types of *Drosophila*, it failed to fit the obvious relative intervals between them, as shown by Bridges (1925). This theory also ignored regulation and assumed a constant difference in quantity rather than in the rate of production of genic material, thus disregarding the factor of time.

According to the third hypothesis, it was supposed that the environment of the embryo might be different, that males were different because they had a different start in life than females, just as in a fern the gametophyte has the characters of a gametophyte because it develops from a spore, while the sporophyte is such because it begins in an archegonium. Thus, in the honey-bee there have been supposed to be two different types of eggs, a female-producing type which cannot develop unless fertilized (obligate zygogenesis)

and a male-producing type which cannot be fertilized (obligate parthenogenesis). It is now generally recognized that, as regards the honey-bee, there is but a single type of egg, facultatively parthenogenetic.

Schmieder (1938), however, suggests that there may be two types of eggs in the chalcidoid *Melittobia chalybii* because of the "fact that the ratio of males to females (3:97), produced by normally mated females, is approximately the same as the ratio of males to unhatchable eggs, produced by females not so mated." These eggs might be female-producing if they were fertilized. However, "unmated females deposit only a very few eggs and of these only a small percentage undergoes development. It is therefore more practicable to study haploid development in eggs obtained from females mated with another species of *Melittobia*, since after such mating oviposition occurs often at a rate comparable to that occurring in normally mated females. The foreign sperm is generally observed as having entered the egg but actual syngamy seems never to occur, or, at least, never to yield viable zygotes. The small percentage of eggs that eventually hatches always produces haploid males of the maternal type." The unhatchability of the eggs fertilized by foreign sperm may be explained by a lethal effect of the foreign sperm, without the necessity of postulating the existence of two different types of eggs. However, if the ratio of males to the few unhatchable eggs laid by unmated females is also only three per cent, then these eggs may be of a different type from those developing into males. They may be unhatchable because unfertilized, or for some other reason, defective yolk, for example. It is stated of *Melittobia acasta* that virgin females lay only a few eggs which invariably develop into males.

In the ichneumonoid *Habrobracon juglandis* it is obvious that at least two-thirds and presumably all eggs are facultatively parthenogenetic. Hatchability may be as high as 98 per cent and viability to the adult stage as high as 90 per cent, whether the females are mated or not. Since two-thirds or more of the offspring from mated females are females, at least this proportion of the eggs which produce males, if unfertilized, would produce females if fertilized.

Results from genetic experiments bearing upon the problem of male haploidy will now be presented.

In the ichneumonoid wasp *Habrobracon juglandis* (*Ashmead*), investigated by the writer and his co-workers, a large number of recessive mutant traits has been obtained, and crosses have been made in various ways to test the principles of heredity. Unmated females heterozygous for various traits produce haploid males segregating in azygotic (approximating gametic) ratios. Linkage tests thus made show that maps are very long in crossover units. On this account, despite much work extending over several years, there is as yet no approximation of linkage groups to the number ten, corresponding to the haploid set of chromosomes.

Recessive females crossed with dominant males produce recessive haploid sons and dominant heterozygous daughters, as expected according to Dzierzon's Law, but if the parents come from the same general stock, then there are also regularly produced diploid biparental males, which resemble their sisters in showing all the dominant traits inherited from both parents, but which are in no way intersexual. They are, however, of low viability and almost sterile. Their sperms are diploid, and consequently their few daughters are triploid. Diploid males are separable from their haploid brothers, not only in showing paternal traits, but also, with some difficulty, by slight differences in proportion of bodily structures and by the larger size of their cells. These are, in fact, much larger than the cells of diploid females. Triploid females, the daughters of diploid males, have cells even larger than diploid females, and haploid males average in cell size almost as large as diploid females (B. R. Speicher, 1935). It appears, then, that while increase in number of chromosome sets increases cell size, maleness as such is an even more effective factor.

Sex determination has been shown to depend upon a series of multiple alleles, of which nine have thus far been identified (Whiting, 1940a, 1943). These are designated *xa*, *xb*, etc., to *xi*. Any heterozygote (diploid), *xa/xb*, *xa/xc*, *xc/xd*, etc., is female; any azygote (haploid), *xa*, *xb*, *xc*, etc., or any homozygote (diploid), *xa/xa*, *xb/xb*, etc., is male. Each member of the allelic series is regarded, not as a single gene, but as a chromosome segment, perhaps very long (to speak in relative terms, because the chromosomes are all very small) and containing many different genes which do not cross over. Thus the allele or segment designated *xa* might have the genes + *Fa mb* + *mc* + etc., *xb* might have *ma* + + *Fb mc* + etc., and *xc* might have *ma* + *mb* + + *Fc* etc.

From these formulae it may be seen that any

heterozygote will show more of the dominant traits determined by the female-producing genes, *Fa*, *Fb*, *Fc*, etc., and that any homozygote or azygote will show more of the recessive traits determined by the male-producing genes, *ma*, *mb*, *mc*, etc. Sex determination is, therefore, complementary, with females of the different genotypes being heterozygous (diploid) dominants for many of the genes, while the males are homozygous (diploid) or azygous (haploid) recessives. Genes for secondary as well as for primary sex characters must be located within this segment. The longer antennae, the larger ocelli, the slightly larger eyes, and the weaker sclerotization of the abdomen of the male must be dependent upon such genes, which in the aggregate are also recessive.

The complementary character of sex determination is shown by certain males which are mosaic for various mutant traits (Whiting, Greb, and Speicher, 1934). Haploid mosaic males occur in a small but variable proportion, less than one per cent, of the offspring of heterozygous females. If a female (black-eyed double dominant) is heterozygous for two different white eye colors, called white, *wh*, and ivory, *oi*, respectively, a son may be produced having the eyes in part white, in part ivory. These two eye colors are normally indistinguishable, but ivory is non-autonomous in mosaics, so that a soaking-through effect takes place from the non-ivory tissue. The complementary action of the two dominant alleles, non-white and non-ivory, then causes a black line in the ivory region edging the white. This black region is "physiologically" double dominant, but genetically ivory. Similarly, in the region of the genitalia of a proportion of mosaic males there are developed small-sized "female" reproductive appendages, indicating a complementary action of dominant female genes in the different sex alleles characterizing the two different types of tissue of the mosaic male. The influence appears to be unidirectional, as if, for example, *xa* affected *xb* but not the reverse. This has, however, not been checked with the different alleles of the series. It was this complementary action in these "gynandroid" males (resembling gynanders) that first gave the hint of complementary sex factors that led to the formulation of complementary sex determination.

Sex determination is, in a sense, polygenic, but because of no crossing-over within the segment, the various groups of genes act as a single series of allelic factors. Just as the many sex-producing genes distributed along the X-chromosome of *Drosophila* segregate as a unit from the Y without crossing-over, so the dominant female-producing genes and the recessive male-producing genes of the *x*-factor in *Habrobracon* segregate without crossing-over from their homologues. Males, as determined by the various alleles (whether azygous or homozygous), or females as determined by any of the heterozygous combinations, are always phenotypically similar.

Whether the sex-alleles ever mutate has not been established with certainty. They appear to be very stable. Extensive breeding within a two-allele stock has resulted in two-allele fraternities almost exclusively, as evidenced by the presence of diploid males. With certain very rare exceptions, the fraternities lacking diploid males are small, so that the lack is evidently due to error of sampling. The rare exceptions, which were not adequately tested, may have been due to contamination, though this is unlikely, or to some combination of factors reducing diploid male viability, or to mutation in a sex allele itself.

In linkage tests, the *x*-factor acts as a single gene, occupying a point on the linkage map. It has been shown to lie near the center of the left arm of a linkage group which is about five hundred crossover units in length. Since no interference occurs across a region approximately central, it may be supposed that the centromere is median.

The method of making a linkage test with the sex-factor may be illustrated by a single example. The mutant gene fused, *fu*, lies about ten units to the right of *x*. An orange-eyed, *o*, female heterozygous for fused, $\frac{o\ xa\ +}{o\ xb\ fu}$, is crossed with a black-eyed fused male, $+\ xb\ fu$. The orange-eyed sons are haploid from unfertilized eggs and are fused and non-fused in equal numbers. Sex linkage is not determinable among these because *xa* males and *xb* males are similar. The black-eyes zygous diploid or biparental offspring, $o/+$, are either fused or non-fused and either males or females. From the ratio of the different combinations, the percentage of crossovers may be determined. In the example given, the non-crossover offspring will be $\frac{xa\ +}{xb\ fu}$ or non-fused females, and $\frac{xb\ fu}{xb\ fu}$ or fused males; the crossovers, averaging in this case 10 per cent, will be $\frac{xb\ +}{xb\ fu}$ or non-fused males, and $\frac{xa\ fu}{xb\ fu}$ or fused females. This is called a two-allele cross.

In crosses involving three sex-alleles, sex-linkage cannot be determined, because all the zygous offspring are heterozygous for x and are therefore female. $xa/xb \times xc$ gives xa/xc, xb/xc. Fecundity is higher here, and the ratio of females to haploid males is doubled, because the poorly viable diploid males are replaced by females. In this case, if the cross is made between a female heterozygous for fused and a fused male, fused and non-fused females are in equal numbers.

Three kinds of fraternities from heterozygous females by fused males are therefore distinguishable as follows:—

	Females	Diploid males	Haploid males
	non-fused / fused	non-fused / fused	non-fused / fused
(1) two-allele.....	9/1	1/9	1/1
(2) two-allele.....	1/9	9/1	1/1
(3) three-allele.....	1/1	0	1/1

Before the multiple allele principle was demonstrated, two different hypotheses, now discarded, were suggested to explain the lack of diploid males in three-allele fraternities. These were differential mutaration, and multiple independently segregating factors (Whiting, 1935).

Habrobracon is an arrhenotokous species, but in combinations of certain stocks unmated females produce a small proportion of diploid daughters in addition to their haploid sons. These impaternate daughters are either heterozygous or homozygous for genes for which their mothers were heterozygous. Female $+/a$ produces impaternate daughters in the ratio: 1 $+/+$, 2 $+/a$, 1 a/a. It was at first supposed that suppression of meiosis, or parthenogamy from a polar nucleus, might cause their appearance (K. G. Speicher, 1934) but cytological work later showed twenty tetrads, the diploid number, in the first oocyte mitosis (Speicher and Speicher, 1938). Two meiotic divisions would then leave the diploid number of monads in the egg nucleus. If the homologues contained different sex-alleles, a female would result. The fact that the heterozygotes for a mutant gene are equal to the homozygotes rather than twice as numerous indicates that the genes, $A\ A\ a\ a$, assort as two pairs, Aa and Aa, rather than at random. This is to be interpreted as meaning that synapsis is initiated in two oocyte I groups of chromosomes which remain separate, and each of which contains both a haploid paternal and a haploid maternal set. The bivalents are therefore paternal-maternal, none being paternal-paternal or maternal-maternal.

If the mother is heterozygous for a sex-linked gene, the 1:2:1 ratio of impaternate females does not hold. Thus when the mother is heterozygous for fused, most of her impaternate daughters are also heterozygous. Most of the homozygotes should be diploid males (Whiting and Speicher, 1935). This example of tychothelytoky in an arrhenotokous species is of interest as showing a possible step toward a thelytokous race.

It will now be of interest to examine forms with haploid arrhenotoky other than *Habrobracon juglandis*, and it will be pertinent to inquire whether there is positive evidence indicating that the *Habrobracon* scheme cannot be universally applied. We may have the possibility of some complementary scheme not involving multiple alleles. Reproductive economy might be low with wastage of fifty per cent of the fertilized eggs (diploid males), as in two-allele crosses of *Habrobracon juglandis*, or good reproductive economy might be attained by some sort of non-randomness in fertilization, by independent pairs of multiple factors, or by some other means to assure that all fertilized eggs should develop into females. Cytologic and breeding data are, unfortunately, indecisive, and genetic evidence is very meager.

We may first consider data from other Hymenoptera and later review the other groups that show haploid arrhenotoky. Two species of *Habrobracon* other than *H. juglandis* have been studied to some extent. In *H. brevicornis*, aberrant males were found (Whiting, and Whiting, 1927) similar to the gynandroid males in *H. juglandis* which gave the hint for complementary sex inheritance. No genetic data were available, but it is probable that these were also gynandroid sex mosaics. B. R. and K. G. Speicher (1940), using a recessive mutation in the same species (rough, similar to rough in *H. juglandis*), obtained from rough females by wild type males, 51 wild type females, 172 rough males, and 34 wild type males. The wild type sons were diploid, as shown by cytologic study, having twenty chromosomes in the spermatocytes instead of ten, the number found in haploid males. These observations may be taken to indicate complementary inheritance.

More extensive data are available for *H. pectinophorae* from the work of Inaba (1939). Diploid males were obtained, identified as in *H. juglandis*, by crossing recessive females with

white eyes or with curved wings to wild-type males. The ratio of females to haploid males was higher in fraternities without diploid males, suggesting three-alleles. Triploid females were obtained, sired by diploid males. Cytological study showed conditions, in general, similar to those in *H. juglandis*, but the chromosomes were evidently somewhat larger and had different spindle fiber positions. Chromosome numbers were as in *H. juglandis*, ten in haploid males, twenty in diploid males and in females, and thirty in triploid females.

These data indicate that cytogenetic conditions in the different *Habrobracon* species are very similar.

In the chalcidoid *Pteromalus puparum*, Dozorceva 1936a, b, c, d; Whiting, 1940b) found a sex-linked trait, red eyes. No fraternities were reported from red-eyes females by wild-type males, such tests for diploid males being lacking. Heterozygous females by red males produced three kinds of fraternities, those with a marked excess of red females, those with a marked deficiency, and those approximating equality. Presumably the first two are from two-allele crosses and the last from three-allele. It is claimed by Dozorceva that there are two types of males, in one of which the sex chromosome is much shorter than in the other, and that females are digametic. As noted above, however, her genetic evidence suggests triple allelels, *xa*, *xb*, *xc*, so that the conditions may be similar to those in *Habrobracon*. B. R. Speicher (unpublished), after examination of *Pteromalus puparum* collected on Long Island, N. Y., was unable to confirm the cytological conditions picture by Dozorceva in the Russian material.

The egg parasite *Telenomus fariai* (Lima), a scelionid wasp of the superfamily Proctotrypoidea, has been investigated cytologically by Dreyfus and Breuer (1944). Daughters and sons of the same mother copulate before leaving the host egg shell, so that there is normally very close inbreeding. Unmated females produce only males. Mated females produce bisexual broods, with females in excess of males. Females have twenty chromosomes, males ten. No diploid males were found. According to the authors, one of the large pairs of chromosomes consists of a V-shaped and a J-shaped member. Males are all haploid, and spermatogonia all show the V. However, the large chromosome that appears in the equal first spermatocyte division is a J. The last spermatogonial division is unequal cytoplasmically. The smaller daughter cell degenerates, while the larger becomes the first spermatocyte. It is assumed that in this division one arm of the V breaks and that the spermatocyte therefore receives a J. The second spermatocyte division is abortive, with a cytoplasmic bud pinched off. Sperms are then all J-bearing. It is further assumed that maturation of the egg is differential, so that the V is always retained. All fertilized eggs then give rise to females, all unfertilized to males. It would be of much interest to have genetic work on *Telenomus* to check with the cytological findings.

Haploidy of the azygous male

The complementary principle of sex determination answers the question, "Why is the haploid a male?" We may now consider the problem, "Why is the male a haploid?" Somatic haploidy has been questioned with respect to the males in many of the arrhenotokous groups. Evidence based on cleavage nuclei is regarded as inadequate (Vandel, 1931a; Schrader and Hughes-Schrader, 1931). We have seen that the drone bee is somatically diploid (in part polyploid) and that this "somatic" diploidy involves most of the germ track.

The difference between the azygote of tychoparthenogenesis and the azygote of haploid arrhenotoky lies in the fact that the former has the diploid type of gametocytes while the latter has the haploid type. The haploid type has become so fixed genetically in the Hymenoptera that spermatogonial diploidy, whether determined by male biparentalism, as in the diploid males of *Habrobracon*, or by regulative doubling, as in the drone honey-bee, fails to result, in the spermatocytes, in the diploid type of mieosis that would produce haploid sperms. The sperms produced are therefore diploid. There is, however, a significant difference in the two cases. Diploid *Habrobracon* males have triploid daughters, because their diploid sperms unite with reduced haploid eggs. The diploidy of the biparental males is "genetic," since their offspring are different from the offspring of haploid males. Diploid drone bees have tetraploid daughters, because the reduced eggs are diploid. Nevertheless, in the bee, eight remains the gametic number because reduction to eight takes place in the oocytes. The diploid drone, therefore, breeds as a haploid although he produces diploid sperms, and the tetraploid queen breeds as a diploid, although she

produces diploid instead of haploid eggs. Eight dyads go to each pole in anaphase and oocyte I and there are eight "monads" in telophase of oocyte II. Whether these "monads" have already become dyads through regulative splitting, and just when the dyads divide to produce a diploid female pronucleus, is a problem for cytological technique and observation. Although somatic diploidy is here extended into the germ track, it does not produce genetic diploidy. If we had been studying genetics rather than cytology, we would not have been aware of this regulative splitting.

The "somatic" diploidy of the drone bee I regard as a secondary type of regulation, subsequent to and probably supplanting the regulation in size of the haploid chromosomes that are seen at least in cleavage and in spermatogenesis of most males in arrhenotokous groups. The drone bee has devised a mechanism extending diploidy into the germ track without surrendering the haploid type of spermatogenesis and his capacity to breed as a haploid. The honey-bee is thus admirably adapted through numerical regulation, so that chromosome numbers in corresponding tissues in males and females are the same. Biparental diploid males of *Habrobracon*, however, are a waste product, of low viability, probably because of their excessively large cells, and producing diploid sperms which are rarely able to fertilize the eggs.

The question asked at the beginning of this section, "Why is the male a haploid?" may be reformulated to read "Why have the males in the groups showing haploid arrhenotoky evolved a haploid type of spermatogenesis instead of retaining the diploid type of the ancestral zygogenetic groups from which they came?"

An answer to this question, which must necessarily be hypothetical, and even somewhat speculative, may be formulated as follows.

The diploid impaternates of tychoparthenogenesis undergo, at some point in their development, delay in cytokinesis relative to chromosome splitting, and thus they become diploid. This may involve a certain disharmony in rhythm between cell body and nucleus. The males of haploid arrhenotoky have had ample time, in the course of evolution, to acquire genes located in the sex differentiating segments, *xa*, *xb*, *xc*, etc., which retard chromosome division relative to chromosome growth. Since these genes are recessive, they do not affect the females (sex heterozygotes) but only the males. Haploidy of the azygous males thus attains regulation of the nucleoplasmic ratio without introducing disharmony between division of nucleus and of cell. Evolution may here have found a more satisfactory solution of the problem of regulation than occurs as an immediate solution in tychoparthenogenesis. Diploid impaternate female grasshoppers, produced by haploid thelytoky, if isolated for many generations, should be found to have advanced cleavage into meiosis so that the oocyte nuclei would become the first cleavage nuclei. Parthenogenesis would then become diploid. Continued endomitosis would result in polyploidy. This is the evolutionary course of azygosis in the female.

The evolutionary course of azygosis in the male must necessarily be different. The azygotic males of haploid arrhenotoky are selected to breed as haploids. This fact sets a limit to their somatic polyploidy.

ORIGIN AND DISTRIBUTION OF MALE HAPLOIDY

Having discussed the various problems involved in the attainment of male haploidy, we may now consider theoretical possibilities concerning the manner in which this mode of reproduction may have arisen. We have seen that parthenogenetic (female) races and species occur with greater or less frequency in diverse groups. This type of constant parthenogenesis has evidently arisen independently many times. It tends toward polyploidy and, as Vandel (1936) suggests, it probably originates from tychothelytoky. Male haploidy, on the other hand, has evidently become established very infrequently, despite the fact that it is a highly successful mode of reproduction possessing the advantages of amphimixis and involving very large taxonomic groups.

The Chromosome Degeneration Hypothesis

Schrader and Hughes-Schrader (1931) have presented a theory of the attainment of male haploidy based on the gradual inactivation or degeneration of one of the haploid groups of chromosomes. This theory attempts to answer the problems of the acquisition of viability in the haploid state and of the regulation of meiosis in the haploid male.

The theory is based largely on work with the homopteran family Coccidae, the scale insects. In this family there exist diverse cytogenetic conditions which, it is suggested, represent "both

beginning and end stages in the establishment of haploidy." In the genera *Llaveia* and *Protortonia* both sexes are invariably diploid, while in *Icerya* and related genera the females are diploid from fertilized eggs, the males haploid from unfertilized eggs. Intermediate stages may therefore be expected, but in no case has a haploid individual been found within the diploid species. However, there is a series of chromosomal conditions of the diploid males themselves which is supposed to represent intermediate stages in the acquisition of haploid male viability. "The basic phenomenon is a gradual degeneration of one haploid set of chromosomes in the male; the process is reflected chiefly in the behavior of the chromosomes during meiosis, but in its late stages the soma also is affected."

The "degeneration" begins in *Llaveia bouvari*, in which about five per cent of the spermatocytes show asynapsis of one autosome pair, while in the remainder pairing is normal. In *Protortonia primitiva*, no pairing of the ordinary sort occurs; asynapsis affects both autosome pairs. There is no evidence for any selective assortment in segregation.

In *Gossyparia spuria*, one haploid set shows marked heteropycnosis, condensing prior to the other set, segregating selectively together and forming a non-functional sperm. The somatic cells, as well, show clumping of this set which appears first during the late blastula stage of the males. There are no heterochromosomes, but males and females are produced by each mother from zygotes which are not morphologically separable in chromosome content. Conditions are apparently similar in the genera *Pseudococcus*, *Lecanium*, and *Phenococcus*. In *Phenococcus acericola* (Hughes-Schrader, 1935) the later stages, showing disintegration of the condensed group, are especially clear. It is concluded that "a physiological haploidy seems to be in progress of evolution through the inactivation of one of the haploid sets of chromosomes in the male line."

Finally, in the iceryine species, the morphological haploidy of the males is completely established.

In the females of all species studied meiosis is of the orthodox type with normal synapsis, with no heterochromosomes, and with two divisions resulting in haploid nuclei.

It is stated that "this scheme attempts no expression of the phylogenetic relations of the various coccid species involved" but "is rather to be interpreted to the effect that some such development as we have traced is taking place in different groups of coccids, but evidently at different rates, and probably in different ways, in different groups.'

The writer doubts that the three cytological departures from orthodox conditions demonstrated in the male coccids-asynapsis, heteropycnotic clumping, and haploid parthenogenesis—are related phenomena.

Asynapsis, involving failure of crossing-over, is of frequent occurrence in other forms. There may later be contact of homologues, a "touch-and-go pairing," so that normal segregation occurs and inviable zygotic combinations are thus avoided, or, as in the asynaptic mutant type in maize (Beadle, 1930), even this brief approximation of homologues may be lacking and chromosomes may be assorted irregularly, so that few gametes contain the haploid set. In none of these cases does there appear to be any association between asynapsis and chromosome degeneration.

There likewise appears to be no association between haploid parthenogenesis and chromosome degeneration. No specific set of chromosomes is "lost" in the formation of haploid males, since any of the possible combinations of chromosomes (or of genes) present in the diploid mother may be transmitted to her haploid sons.

The degenerative processes noted in the male coccids of the Gossyparia type may be compared with conditions in the fly *Sciara* (Metz, 1938). Here a complete and perfectly normal chromosome set received by a male from his mother will,if transmitted to his son, be lost in the monocentric mitosis of the spermatocytes of his son. Just what chromosomes, whether paternal or maternal, are lost in *Gossyparia* is not known, for no genetic factors have been followed to tag them as has been done in *Sciara*.

In *Sciara* all zygotes are similar at the beginning, and sex is determined by differential loss of sex chromosomes in cleavage. This loss depends upon the genetic constitution of the mother which may be a male-producer or a female producer; or, in certain races, both sexes may occur in the same brood. Comparison may further be made with conditions in the isopod crustacean *Trichoniscus provisorius* (Vandel, 1938), in which, as in *Sciara*, there are (monogenic) races with unisexual broods having female-producing (thelygenic) females and male-producing (arrhenogenic) females, and there are (deuterogenic) races with bisexual broods.

(The terms used here indicating sex of offspring imply a special type of zygogenetic reproduction rather than parthenogenetic). Whether in *Trichoniscus* there is chromosome elimination or clumping in cleavage is not indicated, but the breeding data here suggest interesting comparisons with the cytological and breeding data on *Gossyparia* and with the cytogenetic conditions in *Sciara*. Evidence suggests that certain other invertebrates may likewise possess a comparable mode of zygogenetic reproduction showing aberrant sex ratios.

Review of Groups Showing Male Haploidy

In the introductory part of this paper there are listed seven groups of animals with haploid males. The Hymenoptera have already been discussed in some detail, and it is now in order to review the others because of their implications for the evolution of male haploidy.

Iceryini

In the iceryine coccids cytological conditions are very clear. The females have four chromosomes, the males two (Hughes-Schrader, 1930). No diploid males have been found. Unmated females produce males only, mated females produce both males and females. In the cottony cushion scale *Icerya purchasi* (Hughes-Schrader, 1927) true females are lacking, but their place is taken by hermaphrodites which presumably evolved from females. These hermaphrodites may produce further hermaphrodites, either by self-fertilization or by mating with males, but since males are infrequent and the eggs are developed in such a way that they are surrounded by sperms from the same individual, self-fertilization is the rule. According to the complementary hypothesis, only two sex alleles would be involved, and the alternatives are presented of considerable reproductive wastage or some type of non-randomness in distribution of sex factors. "Coincident with or slightly before the first appearance of haploid nuclei in the hermaphrodite gonad, degeneration of certain germ cell nuclei occurs" (Hughes-Schrader, 1927). Whether this is in any way selective is not clear. There is apparently no marked mortality of eggs subsequent to fertilization, but the possibility is not excluded that death of the homozygotes, diploid males, may occur at some post-embryonic stage. This highly successful species may be tending, through hermaphroditism and self-fertilization, toward the eventual complete elimination of males and may be getting on very well in spite of a possible wastage of fifty percent of its eggs.

Haploid males have not been found in other groups of coccids, among which the species are either zygogenetic or parthenogenetic (female). Sex determination in some groups (*Gossyparia*, etc.) may be of the *Sciara* type. In others (*Llaveia*, etc.) it is of the usual XY-male backcross type (Schrader and Hughes-Schrader, 1931).

Aleurodidae

"White-flies" of the homopteran family Aleurodidae are pests in greenhouses and in fruit orchards. The group has been much neglected by taxonomists, and it is probable that the majority of the species is as yet undescribed. In many species males are not known. Zygogenetic species have not been found. *Aleurotulus nephrolepidis* (Thomsen, 1927) is purely thelytokous. Morrill and Back (1911) state that virgin females of *Dialeurodes citri*, the citrus white-fly of the southern United States, produce males only. Thomsen (1927) reports that in *Aleurodes proletella* there is arrhenotoky, and that mated females produce both males and females. All eggs give off two polar bodies, and the unfertilized remain haploid.

In *Trialeurodes vaporariorum* (Schrader, 1920, 1926; Thomsen, 1927) there are two races. An arrhenotokous race occurs in America, in England, and in Denmark. A race reproducing by indefinite thelytoky and with spanandric males occurs in England and in Denmark. Thelytokous females may be mated with males of the arrhenotokous race or with the spanandric males, but it is supposed that the eggs are not fertilized. Maturation is apparently the same in the eggs of both races, with two polar bodies given off and chromosome number reduced from twenty-two to eleven. In the thelytokous race the eleven chromosomes of the reduced egg nucleus probably divide before the first cleavage giving the diploid number, for in the two-nucleus stage of cleavage the chromosome number is twenty-two. Regulation of this type is theoretically reconcilable with the *Habrobracon* principles of sex determination, if it be assumed that the thelytokous race differs from the bisexual due to a chromosomal rearrangement. As I have stated in a previous publication (Whiting, 1943), "homozygous females might be produced if x were duplicated either by translocation or by unequal

crossing-over so that *xa/xb* + 2A would become *xa xb/xa xb* + 2A. Theoretically, haploid females *xa xb* + 1A would then be possible. That such have not been found is not surprising, however, since thelytokous forms tend to become polyploid rather than haploid."

Micromalthus

Among the beetles there are several thelytokous species and races, especially in the Curculionidae (Székessy, 1937; Milliron, 1939; Suomalainen, 1940). Most of these are polyploid. I have found no reference to tychoparthenogenesis in the Coleoptera. Since beetles are of the XY-male type, we might expect haploids to be female.

In males of the aberrant beetle *Micromalthus debilis*, however, Scott (1936) was able to establish somatic haploidy from examination of blastoderm nuclei and tissue of the sternal gland. *Micro malthus*, the only representative of the family Micromalthidae, is the only example of haploidy reported in beetles and also the only beetle showing paedogenesis. Spermatogonial divisions are regular, showing ten chromosomes. The aberrant spermatocytes have already been mentioned. The life history is complex (Scott, 1938), with hypermetamorphic larval stages and with paedogenesis suggesting the cecidomyid flies *Miastor* and *Oligarces*. Normally the larval mother of the adult male deposits a single egg, from which there hatches a "metrophagus" larva. This larva, after devouring its mother, pupates and then ecloses as an adult. Scott (1941) was able to show that if the egg is removed before it hatches, the larval mother will then produce female offspring viviparously. Sex is therefore determined in the process of oogenesis. Normally, females are produced viviparously by paedogenetic larvae. Cleavage nuclei in female-producing eggs have twenty chromosomes. Continuous paedogenesis apparently maintains the species, and it is not known whether the adults reproduce.

No sex chromosomes are differentiated cytologically. The diploids, larvae and the adult female, may be heterozygous for sex as in *Habrobracon*, *xa/xb*. Haploid parthenogenesis would then produce males, *xa* and *xb*.

Thysanoptera

Minute insects known as thrips belong to the Thysanoptera, an order of uncertain affinities. Thelytoky occurs as the usual mode of reproduction in several species, which are members of different families in both suborders. Collections show wide deviations in sex ratios of the bisexual species and differences occur in different seasons of the year suggesting arrhenotoky. Of two species of *Anthothrips*, *niger* is thelytokous, *verbasci* is arrhenotokous, as shown by breeding tests (Shull, 1917), and collections indicate that *Chirothrips manicatus* is arrhenotokous. Breeding tests have likewise shown arrhenotoky in *Franklíniella insularis* (Davidson and Bald, 1931). There is some evidence that bisexual generations may alternate with thelytokous in *Physopus pallipes*, but Vandel (1931a) considers that two different races may exist in this species. There seems to be no evidence that males are produced bisexually, and since arrhenotokous types occur in both suborders, parthenogenesis may exist in all species of the order, with some thelytokous, others arrhenotokous, and with still others having racial types differing in this respect.

Trochelminthes

With the exception of a single family, the Seisonidae, which are marine parasites on the crustacean *Nabalia*, the males of rotifers appear at certain periods only and are presumably impaternate and haploid. The phylum Trochelminthes is characterized by irregular heterogony or alternation of generations (Shull, 1929; Whitney, 1929). Females are of two types, gamic and agamic. All eggs produced by agamic females give rise to females parthenogenetically and cannot be fertilized even though mating may take place. Males appear at certain times and much work has been done in attempts to determine the various causes of male production. In addition to genetic differences in strain or species, factors are temperature, nourishment, and chemical composition of the water. Since agamic females cannot produce males, these factors do not cause the production of males directly, but rather the production of gamic females. If gamic females do not mate, their eggs develop immediately into males by haploid parthenogenesis. Fertilized eggs do not develop for a considerable time. They accumulate reserves of nourishment and a thick shell to tide the species over unfavorable conditions. Eventually they give rise to females, which are always agamic, but the next generation may consist of gamic and agamic females. Eggs of agamic females give off but one polar body and

produce females by diploid parthenogenesis. Eggs of gamic females form two polar bodies whether they are fertilized or not.

Some rotifers have but one sexual period a year, usually in the spring, others have one in the spring and a second in the fall, while still others have many sexual periods, the number of which may vary according to locality. Males are unknown in the Bdelloida and in species of several other groups, so that these forms reproduce by constant thelytoky.

The more primitive rotifers are crawling types, thus showing some resemblance to their possible ancestors, the Turbellarians (Wesenberg-Lund, 1923, 1930). In these rotifers the males are as large in size as the females. Evolution seems to have been in the direction of planktonic habit and of smaller size of males, with correspondingly greater numbers produced in the sexual period. There is no parasitism of the male on the female, but structural degeneration is carried so far that the male may be nothing but a sperm sac, surrounded by protoplasm bearing a tuft of cilia which enables it to reach the female. The sperms in this case are already fully formed before the male emerges from the egg, and the male mates immediately after hatching. Hermaphroditism is lacking in the phylum. In the family Notommatidae, regarded as the primitive type from which most, if not all, of the other families may be derived, very few males are known, but these are in size similar to the females.

The Seisonidae, mentioned above, are said to depart from the rotifer type, in that males are as abundant as females and can be obtained at all seasons. They are supposed to be produced bisexually, and it is stated that there is no parthenogenesis in the group. The single reference to spermatogenesis (Illgen, 1914) does not indicate whether the males are actually diploid and produced bisexually. If this be true, it suggests that a group which has attained haploid arrhenotoky may revert to the usual method of sexual reproduction. However, the family is somewhat isolated taxonomically, is primitive in the sense that the males are equal in size to the females, so that its origin may be separate from that of the other rotifers. A third possibility is that this family has retained haploidy but has given up heterogony. Without breeding tests or cytological study of the spermatocytes, it cannot be stated that Dzierzon's Law does not apply to this group.

Acarina

The Acarina, mites and ticks, are the only arachnids in which parthenogenesis has been proved with certainty. Sokoloff (1934) investigated spermatogenesis in six species of the Parasitidae (Gamasidae) showing tetrads with normal division. André (1935) discusses this family and concludes that, despite several supposed cases of parthenogenesis (thelytoky). none has been proved.

André (1935) also discusses the ticks (Ixodoidea) and concludes that thelytoky has been established only in *Amblyomma*. In *Hyalomma aegyptium*, Tuzet and Millot (1937) show a large body in the first spermatocyte which they interpret as a heterochromosome. It is probably a sex chromosome but is not illustrated in their drawing of spermatocyte II and should be further investigated. In *Argas columbarum*, Oppermann (1935) shows a large XY pair, in addition to twenty-four autosomes, in the spermatogonia. The autosomes synapse to form twelve tetrads which divide in the usual manner. Division of spermatocyte I is equational for XY, and division of spermatocyte II is reductional. In addition to twenty-four autosomes, the two X-chromosomes are shown in the oogonia.

André (1935) regards the observations made on *Syringobia chelopus* of the Sarcoptidae as inexact and as not establishing thelytoky. In the Cheyletidae, there have been several supposed cases, but only one proven. *Cheyletus eruditus* Schr. is regarded as thelytokous, but Cooper (1937) states that the possibility of its being a self-fertilizing hermaphrodite has not been excluded, and the same applies also to the tick *Amblyomma dissimile* Koch.

Evidence for arrhenotoky in mites is much more convincing. Putnam (1939) states that unmated females of the plum nursery mite (*Phyllocoptes fockeui*), of the Eriophyidae, produce males only. Pätau (1934, 1936) has shown that the grain itch mite *Pediculoides ventricosus* is arrhenotokous, with three chromosomes in the spermatogonia. He has investigated cytological conditions in the egg. The three bivalent chromosomes of the oocyte were followed from pachytene through the growth stage and appear as either rod or ring tetrads in metaphase. There are two meiotic divisions. In early cleavage telophases, the chromosomes form separate karyomers. Pätau

discusses the application of the *Habrobracon* scheme of sex determination, but at the time his paper was written he did not have available the multiple allele principle.

Cooper (1937, 1939) has shown that the grass mite, *Pediculopsis graminum*, is arrhenotokous. His cytological studies revealed conditions very similar to those in *Pediculoides*, with three as the haploid number, six as the diploid, and with karyomeres formed by chromosomes in cleavage. *Pediculoides* and *Pediculopsis* are very closely related genera of the family Tarsonemidae.

Schrader (1923) showed arrhenotoky in the "red spider" *Tetranychus bimaculatus*, with three chromosomes in spermatogenesis, and with three pairs of chromosomes in the female forming tetrads in oocyte I. Cleavage is shown in the unfertilized egg, with three chromosomes, and in the fertilized, with six that form karyomerites in the resting stage. Other species of Tetranychidae mentioned as arrhenotokous are *Tetranychus telarius*, *lintearius*, and *ulicis*.

The Acarina are exceedingly diversified in modes of life, structure, and developmental conditions. There is great disparity in the method of grouping the various families, so that selected species may be far separated or not, according to the system adopted. If the arrangement suggested by Ewing (1934) be followed, placing the Parasitoidea and the Ixodoidea together in the Mesostigmata, which is one of the twelve suborders, then the forms reported as zygogenetic, having normally diploid biparental males, will be grouped together. How many other mites are also zygogenetic, and how widely spread is arrhenotoky, remains to be investigated. Possibly arrhenotoky has arisen and become established but once in the order, which may suggest that the primary phyletic division of the Acarina should be on that basis.

Thelytoky appears to have developed many times and in species of diverse acarine groups. Its incidence probably has no relationship to the more general and basic method of amphimixis, whether that be haploid arrhenotoky or zygogenesis. As for the method of sex determination, the XY shown by Oppermann for *Argas* is the only convincing case. Other investigators seem not to have been interested in this problem, except Sokoloff, who fails to find sex chromosomes in the Parasitidae (Gamasidae).

General comparisons

Male haploidy as a mode of amphimixis appears to have arisen in different groups of Metazoa from the more usual zygogenetic mode, in which males as well as females are normally diploid and biparental. If we except the very aberrant beetle *Micromalthus*, there is no evidence at present available to show that this change has occurred independently more than five or six times. One of these took place in an ancestral rotifer and may now involve the entire phylum Trochelminthes. Possibly it was a single ancestral mite that gave rise to a stem with many arrhenotokous branches, while another conservatively retained zygogenesis. Whether male haploidy has arisen independently more than once in the Acarina is left for future experiments and observations to decide. Evidence is as yet weak, but there appears to be nothing to prove the existence of a zygogenetic thrips, so that we may imagine a single ancestor of the whole order Thysanoptera as having departed from the mode of his now unknown relatives. The same applies to the Hymenoptera, but here we may speak more positively, in view of the wide scatter and considerable amount of breeding data and cytological study devoted to the order.

Of the two homopteran families showing male haploidy, the aleurodids have no known zygogenetic species. Among the coccids, on the other hand, of the seven subfamilies only the Monophlebinae include known arrhenotokous species. These make up the tribe Iceryini but are absent from the Llaveiini, which are all diploid, and the males of which have XO chromosomes. According to one theory, the aleurodids have been derived from the coccids. They may have descended along with the iceryines from some common ancestor which adopted male haploidy.

This would total but five known independent phyletic arrhenotokous pedigress, with *Micromalthus* as a lone and inconspicuous sixth. Doubtless, other instances may be found, but the ratio with respect to constant thelytoky is likely to remain very low, for new examples of the latter are frequently appearing in the literature.

The change to haploid arrhenotoky has evidently occurred very rarely and has come to involve large taxonomic groups as a stable mode of reproduction. Whether male haploidy may ever revert to zygogenesis is not known. As a test

for this possibility, search for normally diploid males should be made in arrhenotokous groups.

The change to thelytoky has occurred and is occurring a great many times in nature, and involves only single scattered species or races. Their adoption of this mode is probably relatively recent, as evidenced by their isolation and by the occurrence of spanandric males among them. Constant thelytoky involves the denial of the advantages of amphimixis and no large or successful group has evolved with this handicap. Only by heterogony, the alternation of a bisexual with one or more parthenogenetic generations, can thelytoky be widely incorporated as a permanent mode of life. Thus thelytoky may be alternated with haploid arrhenotoky, as in the rotifers and the gall-wasps; and thus thelytoky, and in this case also diploid arrhenotoky, may be alternated with a zygogenetic bisexual generation, as in aphids, phyloxerans, and cladocerans. In these three groups a generation of impaternate diploid males and females alternates with one or more generations of agamic females.

Race-hybrid Hypothesis

Tychothelytoky in groups of the XY-male type or tychodeuterotoky in groups of the WZ-female type may be regarded as the most primitive form of natural parthenogenesis. It appears merely as a breakdown of the block to post-meiotic mitosis which has been established in animals but which is absent from plants. Being haploid in origin, it encounters the hazards both of low chromatin content and of recessive lethals, and must undergo both quantitative regulation and stringent selection, respectively.

Parthenogenetic (female) races and species have probably originated from tychothelytoky (1) by oogonial tetraploidy, (2) by advancing cleavage into meiosis so that an oocyte nucleus becomes a cleavage nucleus, (3) by parthenogamy from a polar nucleus, (4) by endomitosis following reduction, or (5) by parthenogamy of cleavage nuclei. The first three of these alternatives involve diploid parthenogenesis with zygosis of the impaternate offspring. The last two involve haploid parthenogenesis with production of a diploid azygote. Chromatin rearrangements, genic mutations, and frequently polyploidy may produce such changes that crossing with the bisexual form becomes ineffective. Sterility and other deviations of the spanandric males from their ancestral type are probably due to these cytogenetic shifts, which modify the original genic balance of the sex chromosome in relation to the autosomes.

At first thought, it may seem that tychoarrhenotoky should logically furnish a first step toward constant arrhenotoky, just as tychothelytoky appears to be the first step toward constant thelytoky. It might be expected that tychoarrhenotoky should occur in groups with WZ-females, if lack of Z should prove lethal. Possibly such a condition may be found among the primitive Lepidoptera or among the related Trichoptera, if these have not acquired by duplication an "autosomal" Z which could render WW females viable. These insect orders, however, constituting a very large and flourishing WZ-female tree, have given rise to no known arrhenotokous branches. If haploid males have various modifications of the factor Z, male haploidy should arise from WZ-female stock rather than from XY-male stock. As regards the mites, coccids, and *Micromalthus*, the cases in which close zygogenetic relatives of the arrhenotokous branches are known, these relatives are of the XY-male type. The suggestion that male haploidy involves female digamety and is thus related to the WZ-female type also meets with the difficulty that through protection of W by Z there has come about a degeneration of the former, so that "backcrossing" becomes fixed as long as WZ functions as the sex differentiator.

The origin of male haploidy through tychoparthenogenesis appears improbable, because few offspring are produced, and these must cross with the zygogenetic stock, thus dispersing the parthenogenetic tendency. A theory involving repeated and extensive race hybridization seems more probable, as furnishing abundant material for natural selection.

Probably modifications within the X-chromosome or within some autosome have occurred, to produce a multiple allelic series and the complementary type of sex determination associated with haploid males. The spanandric males of geographic thelytoky presumably have chromosomes that have undergone modification during their period of protection in the parthenogenetic stock. However, the infrequency and predominant sterility of such males would render infinitesimal the chance of success for any of their few progeny surviving the stringent selection leading toward the goal.

What is needed for the evolution of male haploidy is a large population with a strong tendency toward parthenogenesis and with modified chromosomes and normal chromosomes freely recombining and crossing over and thus originating new types. Such a population might be found in the borderlands between the ranges of an ancestral zygogenetic species and of its thelytokous offshoot. To be specific, let us imagine some ancestral hymenopteran species, some protosiricid of the Early Jurassic for example, in which the zygogenetic stock has already given rise by tychothelytoky to a parthenogenetic (female) scion. The latter, breeding in an isolated territory, has multiplied over a period long enough to accumulate several genetic modifications, but still brief enough for retention of crossability with the parental stock. Now, with increasing population, the areas of stock and scion come to overlap and crossing occurs, at first between the males of the zygogenetic stock and the parthenogenetic females, then between the former and the hybrid females, and later between the members of the segregating fraternities. Thus there is furnished abundant opportunity for cytogenetic recombinations. In this population we might expect great numbers of inviable types to be produced and, among the small proportion of viables, there should be many sterile intersexes. Arrays of genetic types would vary from those needing syngamy to initiate cleavage, at one extreme, to parthenogenetic forms at the other, and from femaleness to maleness.

The viable fertile types should be stabilized at three modes, (1) zygogenesis of both sexes, (2) parthenogenesis of females with males eliminated, (3) zygogenesis of females, parthenogenesis of males. Theoretically, fertile hermaphroditism might be a fourth mode, but this has not been found as a primitive condition in any of the groups which we are considering. Modes (1) and (2) would be reversionary. Mode (3) would furnish the basic material from which the stable arrhenotokous type might evolve. This third type would be produced from combinations in which factors for maleness were recessive, and genes determining parthenogenesis were present. Thus the zygotes would tend to include the females, while the azygotes would be male.

With multiple factors for sex segregating independently, there would be considerable reproductive wastage because of sterile intersexuality. This would tend, by selection, toward a concentration of the effective sex genes into a single closely linked group.

Regulative somatic diploidism of the impaternate males might, for a time, permit normal (diploid) meiosis in spermatogenesis, but since the azygotic embryos would originate as haploids, there should be gradual selection for an increase in chromosome size as a regulative process. This should tend to retard numerical regulation so that, eventually, the entire embryo might become haploid. Since tissues of the germ track are relatively undifferentiated, the spermatogonia especially should retain the initial haploidy of the embryo. The spermatocytes, furnished with the haploid set of chromosomes, would then be faced with the necessity of modifying their type of division, and these modifications should be perfected and standardized by selection.

Genes for enlarged cell size should tend to be concentrated along with the genes for maleness and, like them, should be recessive. Thus the haploid males should come to have cells approximating in size the cells of the females, but the diploid males should have much larger cells, for, to the homozygous recessive genes, there should be added the double amount of chromatin. Diploid males should then be reduced in viability. In *Habrobracon*, it is known that the cell size of diploid males is abnormally large, and there is some evidence that among diploid males larger cell size is associated with higher mortality.

As reduction in spermatocyte meiosis initiated by selection in the haploid males becomes characteristic of the species, the diploid males might also be rendered sterile, as they are in *Habrobracon*. Fixation of the meiotic condition adapted to haploid males should effectively eliminate the few surviving diploid males from reproduction.

Evolution of the sex differentiating segment, called the *x* factor in *Habrobracon*, must have come about by reduction in crossing-over within it and by mutation and the selection of minor factors, so that the various alleles, *xa*, *xb*, *xc*, etc., should come to produce a similar phenotype. Genes governing secondary, as well as primary, sex differences must lie in this chromosome segment, the genes for male traits acting as recessives.

Additional alleles in the series might be brought about as follows: a new allele might originate as some repeat of a local duplication, which would at first give a phenotype somewhat different from the

preceding, but this should later be perfected by minor mutations and selection. Whatever disadvantage a new allele might have, because of its production of a divergent phenotype, would tend to be compensated by a selective advantage gained by increasing the number of alleles in the population. Thus in a three-allele population, with the older perfected alleles, *xa* and *xb*, relatively more numerous, these alleles are at a disadvantage because half of the fertilized eggs are wasted (inviable or diploid males), but the matings involving the third allele, *xc*, are much more fecund, even if it be supposed that the females *xa/xc* and *xb/xc* and the males *xc* are somewhat divergent or defective.

In species that are highly differentiated sexually, there may obtain much genetic diversity which does not express itself phenotypically. In this, also, genotypic sex divergence may be greater than genotypic sex convergence without becoming evident. In other words, intersexuality is more obvious than supersexuality, as has been demonstrated in many Lepidoptera, *Lymantria* for example. Seiler's WW females, also, are not only viable but apparently non-separable phenotypically from WZ females. Doubling the genotypic divergence from males, ZZ, produces no visible difference.

It is of interest to note that in all the groups characterized by haploid arrhenotoky, there is a very high degree of sex disparity. The gonadic hermaphroditism of the coccid *Icerya purchasi* appears to be, not a step toward intersexuality, and certainly not in any way primitive, but a secondary modification of a highly differentiated female type, enabling the species to maintain itself and, at the same time, dispense with its highly differentiated males. The sex alleles in haploid arrhenotoky may then differ genically to a considerable extent among themselves without producing perceptible differences. This should aid both in the inception of male haploidy and in the evolution of new alleles in arrhenotokous species.

No phenotypic differences caused by the different sex alleles of *Habrobracon* have yet been demonstrated with certainty. In one inbred stock the males had more antennal segments and the females fewer than in another inbred stock (Whiting, 1943). This indicates the association of genic differences governing antennal segments with the *x*-factor. The apparent unidirectional feminization in gynandroid mosaic males suggests a difference in autonomy of similar structures determined by different alleles.

It is suggested that the various steps outlined above may have brought about the stable condition of amphimixis known as haploid arrhenotoky, not only in the Hymenoptera, but also, at an earlier or later period, in the evolution of the other five or six groups mentioned.

SUMMARY

Among invertebrate animals belonging to seven different groups, a mode of reproduction has been established in which males are haploid, developing parthenogenetically from reduced unfertilized eggs. Five problems involved in the attainment of male haploidy are considered.

(1) Induction of parthenogenetic development in eggs is regarded merely as a breakdown of the block to post-meiotic mitosis which is normally established in animals but is absent from plants.

(2) Viability of the azygote resulting from this initial parthenogenesis is attained by a regulative doubling of chromosome number that corrects the nucleoplasmic ratio, and by selection against recessive lethal factors. The few surviving azygotes are diploid with normally diploid meiosis. Because of their infrequent and scattered occurrence their production has been called tychoparthenogenesis.

(3) Meiosis in the haploid male has been modified in different ways in different groups. This has come about by a selective process when the spermatocytes have received a haploid set of chromosomes.

(4) Sex determination in these groups is regarded as complementary, depending upon a single series of multiple alleles having similar effects phenotypically. Each allele is considered a chromosome segment containing many genes, dominant female-producing and recessive male-producing. Females are heterozygous, normal haploid males are recessive. The homozygous recessive combinations (infrequent because of multiple allelism) are inviable or may occasionally develop as diploid males.

(5) Haploidy of the azygous males may be defined as having the haploid type of spermatogenesis, with chromosome reduction lacking. Somatically, these males may be haploid, at least in early developmental stages, or they may have secondary diploidy or polyploidy, as in the drone of

the honey bee, which begins as a diploid because of precocious splitting of the ootid monads.

The hypothesis of the gradual degeneration of one set of chromosomes in the attainment of haploid male viability and in the regulation of meiosis, advanced by Schrader and Hughes-Schrader (1931) on the basis of their studies of coccids, is questioned. It is considered that their postulated stages in chromosome degeneration (asynapsis, heteropycnosis, and male haploidy) are independent phenomena.

Male haploidy is apparently characteristic of the entire insect order Hymenoptera, and probably of thrips and rotifers. A single beetle, *Micromalthus*, has haploid males. It is suggested that the primary phyletic division of the mites, Acarina, may be made on this basis. Among the coccids, the tribe Iceryini are characterized by male haploidy, and it is possible that the related homopteran family Aleurodidae may be derived in common with this tribe.

According to Vandel (1936) parthenogenetic (female) races and species have originated from tychoparthenogenesis by extension of the process of chromosome doubling into meiosis, so that the type of parthenogenesis becomes diploid from the beginning, and a fixed mode of reproduction. Further doubling lies at the basis of the polyploidy characteristic of many parthenogenetic races.

A review and discussion is given of some of the recently published cases of tychoparthenogenesis and of the fixed parthenogenesis of unisexual (female) races and species.

As a possible explanation of the origin of male haploidy, an hypothesis is suggested of hybridization between a normal bisexual race and a parthenogenetic (female) race in which the chromosomes have become modified. Among the segregating types resulting from this hybridization there should be azygous males. Nucleoplasmic regulation in these males should be gradually modified by an increase in the size of chromosomes, so that the haploid number might be retained into progressively later embryonic stages, especially in the germ track. In this way meiosis of the spermatocytes should be changed from the diploid to the haploid type.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to B. H. Willier, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *Department of Biology, Homewood Campus, The Johns Hopkins University, Baltimore 18, Maryland, U. S. A.*

EVOLUTION: A SYNTHESIS OF GENETICS AND PALEONTOLOGY.

Being a review of *Tempo and Mode in Evolution*, by *George Gaylord Simpson*. New York (Columbia Biological Series Number XV, Columbia University Press). $3.50. xviii + 237 pp. 1944.

By Bentley Glass, Goucher College

The study of the causes and mechanisms of evolution has in the past decade received a fresh and profound impetus through the application, on the one hand, of the statistical theories developed chiefly by Fisher, Haldane, and Wright, and on the other, through the application of experimental methods to the study of evolutionary changes. The publication of Dobzhansky's *Genetics and the Origin of Species* in 1937 marks the beginning of this new era in many minds, for that book first unified the new concepts and presented a stimulating and well-ordered survey of evolutionary change on three levels, those of the raw material—gene and chromosome mutations; the action of natural selection and other factors determining the fixation or loss of genes in populations; and the influence of isolation, geographical and genetic, in perpetuating population differences and establishing species. The widening influence of this book and of the mathematical theories of evolutionary change has been reflected in recent years in a number of books of outstanding significance. *The New Systematics* (ed., Julian Huxley) and Mayr's *Systematics and the Origin of Species* reveal the invigorating effect of new ideas upon old-style taxonomy, and have blazed new trails for investigators to follow. Darlington, in *The Evolution of Genetic Systems*, has, in brilliant speculations based upon cytological data, outlined aspects of evolution heretofore little considered. Goldschmidt, in *The Material Basis of Evolution*, has developed radical ideas about the origin of species and higher systematic categories. Julian Huxley has assembled and collated an enormous mass of data bearing on the varied modes of speciation in *Evolution: the Modern Synthesis*, and has developed his original ideas of progress in evolution and the significance of dominant forms.

The most recent addition to this group, *Tempo and Mode in Evolution*, is fully worthy of their company and may well be ranked with the best of them. Certainly no scientific book in recent years has kept the present reader so continuously excited that each fresh page seemed an adventure, each new chapter an exploration of a Terra Nuova. There is good reason for this feeling. Between no branches of evolutionary biology has the chasm yawned wider than between paleontology and genetics. The geneticist has been prone to regard the paleontologist as a misguided believer in mystic orthogenesis, whose sole contribution to biology was the long-since demonstrated truth of evolution. The paleontologists, on the other hand, in Simpson's own words, "felt that a geneticist was a person who shut himself in a room, pulled down the shades, watched small flies dissporting themselves in milk bottles, and thought he was studying nature. A pursuit so removed from the realities of life, they said, had no significance for the true biologist." There has been no one competent enough in both fields to bridge the gap between them. Yet there are common problems which are only capable of solution through combined attack. In particular, as Simpson points out, the study of evolutionary rates and patterns requires such an approach. It is these aspects of evolution that Simpson considers under the headings "Tempo" and "Mode."

The treatment is therefore by design a limited one. This is neither a large nor an exhaustive book. Its genius lies, in the first place, in the application of genetic concept to paleontological data; in the second place, to the grasp and originality of Simpson's thought. It is not too much to say that hereafter no treatise on the causes and modes of evolution can ignore it, no paleontological interpretation proceed from previous bases. As a geneticist, the reviewer is filled with admiration of the clear understanding and able exposition

of genetic thought manifested by the author to a degree that makes one feel somewhat envious. To a biologist only slightly acquainted with the data of paleontology, on the other hand, the effective, intelligible way in which the author selects and presents the material from his own field is equally admirable.

In an introductory chapter, Simpson deals with rates of evolution—rates absolute and relative, rates in genetically related unit characters and in genetically independent characters, correlative rates, rates of single characters, of entire organisms, and of groups. Illustrations are drawn chiefly from the evolution of the Equidae, establishing the theses that "(1) the rate of evolution of one character may be a function of another character and not genetically separable, even though the rates are not equal; (2) the rate of evolution of any character or combination of characters may change markedly at any time in phyletic evolution, even though the direction of evolution remains the same; (3) the rates of evolution of two or more characters within a single phylum may change independently; (4) two phyla of common ancestry may become differentiated by differences in rate of evolution of different characters, without marked qualitative differences or differences in direction of evolution." Rates correlated with an external factor (thickness of strata) are illustrated from the ammonite *Kosmoceras*. Rates of evolution of entire organisms as units are found to be approximately equal in chalicotheres and horses, while those of ammonites are much slower. Survivorship curves for genera of pelecypods and land carnivores are found to be different, and, for the Carnivora, the number of genera existing at any one time, the rate of origin of new genera, and the rate of disappearance of genera are positively correlated with one another, and presumably with rate of evolution.

The next topic is that of the "Determinants of Evolution." Variability, rate of mutation, character of mutations, length of generations, size of population, and selection are considered with thorough grasp of genetic thought. Simpson's criticism of Goldschmidt's view that "systemic" mutations are of prime importance in the origin of species and higher categories is one of the most cogent that has been offered. His application of the mutation theory to paleontologists' disputes over the continuous or discontinuous origin of new characters is convincing. An unfortunate series of typographical errors should be noted here. On pp. 66–68, in discussing the effects of population size, a number of formulae are written $1/2\ N$, $1/2\ s$, $1/4\ \mu$, etc., instead of the correct forms $\frac{1}{2N}$, $\frac{1}{2s}$, $\frac{1}{4\mu}$. This may create considerable difficulty in following the argument, for all those not aware of the error.

Simpson next distinguishes between micro-evolution ("changes within potentially continuous populations"), macro-evolution ("rise and divergence of discontinuous groups"; "rise of species and genera"), and mega-evolution ("large-scale evolution studied by the paleontologist"; "differentiation between families, orders, classes, and phyla"). The primary question here is that of the nature of the discontinuities that become more and more common at higher levels. It is shown that minor discontinuities can be referred with probability to stratigraphic hiatuses, or to sampling of migrants instead of main lines. On the other hand, the major discontinuities, as strikingly exemplified in the origins of the mammalian orders, remain to be accounted for. Here the author develops his major original contribution to theory. He proposes that "mega-evolution normally occurs among small populations that become pre-adaptive and evolve continuously (without saltation, but at exceptionally rapid rates) to radically different ecological positions."

The author then passes to the examination of low-rate (bradytelic) and high-rate (tachytelic) lines, developing the thesis that both of these arise from normal-rate (horotelic) lines that vary in normal frequency fashion, so that extreme variant horotelic lines are with difficulty distinguished from the true low-rate and high-rate lines. There are numerous classic examples of low-rate lines. Simpson suggests that these normally have large breeding populations, arise through rapid evolution, are predominantly and closely adaptive in an ecological situation that is only slightly or periodically variable. The high-rate lines, on the other hand, "either become extinct or usher in new major adaptive grades." The significance of relicts is also discussed.

In a chapter, "Inertia, Trend, and Momentum," the phenomenon of rectilinear (orthogenetic) evolution is taken up, and again the trends in the Equidae furnish the main evidence. The conclusion reached will satisfy geneticists but probably not all paleontologists: "A dispassionate survey suggests that much of the rectilinearity of evolution is a product rather of the tendency of the minds of scientists to move in straight lines than of a tendency of nature to do so." Of course, Simpson also recognizes the possibility of orthoselection and of secondary trends correlated with a selective trend. Momentum effects he regards as only apparent, interpretable on a variety of genetic bases.

The remainder of the book is hardly more than a sketch of Simpson's theoretical thinking. It contains two chief ideas which are briefly developed. First, animals occupy adaptive zones of the environment, but occupation of a zone depends on the preadaptation of the environment and of the organism, as well as the zone's vacancy or occupancy only by a less well adapted group.

Secondly, there are three main modes of evolution. Speciation takes place within an adaptive zone, from subzone to subzone. Phyletic evolution takes place within one adaptive zone as it slowly changes character. (This resembles Huxley's successional species.) *Quantum evolution* takes place in the step-like shift from

one adaptive zone to another, giving rise to distinctly new adaptive types. It may be divided into an inadaptive phase, a preadaptive phase, and a final adaptive phase. It is illustrated by the origin of hypsodonty in horses. Populations in quantum evolution are unstable, small, tachytelic, with a rectilinear mode of change of short duration. The small population and the rapidity of the change account for the hiatus in the fossil record. The concept of quantum evolution Simpson regards as the most important, if also the most controversial and hypothetical one that he has advanced.

Some readers will undoubtedly feel that the supporting evidence for the volume of theory is rather thin. Simpson would probably agree. From intent, the discussion has been kept free of the superfluity of examples which in many books of this sort chokes the flow of thought, obfuscating rather than clarifying the central ideas. Where there is a genuine paucity of evidence, thinking such as this, soundly based on what is known, is the one thing most likely to stimulate the research that will produce evidence, either for or against. No student of evolution, from this day forth, can afford to be ignorant of *Tempo and Mode in Evolution*.

REVIEWS AND BRIEF NOTICES

GENERAL BIOLOGY: PHILOSOPHY AND EDUCATION

PROCESS AND POLARITY. *Woodbridge Lectures Number 1, Delivered at Columbia University 1943.* *By Wilmon Henry Sheldon. Columbia University Press, New York.* $2.00. xvi + 153 pp. 1944.

This book offers four things of general interest: (1) a theory of truth; (2) a critical analysis of important world-views; (3) an attempt to show that polarity and "process" are pervasive aspects of reality; and (4) side-lights on the general nature of certain things studied by the special sciences.

Sheldon's theory of truth may be boiled down to something like this: just as the good for an animal is whatever adapts it to its environment, so the true for man is whatever in the way of belief brings success. For Sheldon, "success" means survival, the completion of intended acts, and living more fully and intensely. Thus the pathway to truth is empirical, pragmatic, and experimental. To him, theoretical tests of truth, such as inclusiveness and consistency, are of subordinate value, and the common error of the past has been the dependence on theoretical tests chiefly or exclusively.

Of the many "world-maps" which philosophers have produced, he thinks that five have satisfied the practical test of truth and have therefore survived: materialism, idealism, scholasticism, process-philosophy, and irrationalism. But no one of these is sufficient. Sheldon characterizes each and comments on its merits and its defects.

Sheldon's own philosophy might be called a sixth type, since it stresses both polarity and "process" as pervasive characteristics of reality. Things are groups of polars; and polarity is itself in a polar relation to "process." Two things are in a polar relation when they satisfy four conditions; they must be: (1) opposites; (2) balanced; (3) to a degree cooperative; and (4) to a degree independent. Sheldon offers a detailed analysis of only what seem to him the three most fundamental of polar pairs: (1) body-mind; (2) one-many; and (3) structure-process.

Sheldon thinks that the greatest discovery of modern times is "process": the existence of irreversible change in which the old is preserved and an increment of novelty emerges, so that there is a progress toward fullness of being. Change thus becomes "an efficient agent in itself," an eternally creative power, and not merely a phase of substance or a stage in the development of substance, and still less just a replacing of one substance by another. Also time—because it is the essence of change—comes to be regarded, like change, as a positive and ultimate principle. In a lyrical passage on the role of "process," Sheldon writes: "Process is nature's great remedy, the healing potion supplementing the imperfections that mar the polar order, the hope and lure of the future and the basis of a working confidence in progress. It is the office of the process-principle to remove the clash and conflict between the polar opposites, restoring their energy for the free variation they should have unmolested. And this free variation means more novelty, ministering in turn to the phase of process. . . ." Reality, for Sheldon, may be diagrammed as a growing spiral with ever larger circles—a logarithmic spiral whose timeless equation defines its indefinite expansion.

Unlike Bergson, Sheldon finds irreversible change in the inorganic world; e.g., the motions of electrons in a current, and heat radiation. On the level of minute phenomena, natural laws are statistical laws, i.e., laws of probability or chance (in its modern meaning). Nature is imprecise, always deviating from exact law. Elementary possibles occur with equal frequency, and gradually every possible and combination of possibles has its opportunity to exist as the process approaches an even distribution. An emergent factor is obvious.

The differentia of living things, according to Sheldon, are: (1) self-perpetuation, (2) growth, and (3) reproduction. While self-perpetuation represents reversible change, growth is normally irreversible, especially where it involves a hierarchy of levels; and reproduction and the evolution of species are also irreversible. Heredity follows a statistical law which indicates that all possible combinations of genes tend to occur with equal frequency. This means "process" with its

incremental variation. Structure and behavior, both ontogenetically and phylogenetically, are in a polar relation to one another, as are also plant life and animal life. He regards the differentia of the mental as: (1) the tendency to look before and after—the ability to recall the past and plan the future; (2) the desire to seek some goods for the future, or aspiration; and (3) a totality, or synthesis, which in some measure determines its details or parts. The progress of knowledge is irreversible, both in the person and in the accumulation of race experience. Knowledge-seeking and end-seeking are polar opposites.

Sheldon's study should probably be read primarily for its suggestiveness. Readers who are looking for consistency, clear cut definition and incontrovertible argument will be disappointed. Indeed, Sheldon goes out of his way to castigate those whose first concern is precision and non-contradiction. "Better an ounce of significance than a pound of precision" is his motto. This is part of an irrationalism which shows itself at times in his pages and which includes also: (1) a subordination of the rational criteria of truth to the practical criterion of "success"—with an occasional acceptance of "direct insight"; (2) a depreciation of "thought"; (3) a denial of the ability of science to "account for" anything; (4) the attribution of philosophical preferences to non-rational factors; (5) a tendency to equate the essence of the real, true, and good with sheer power (the reservations inserted to avoid being driven from this position to an approval of egoism, power politics, etc., being left undefended). An important consequence (or cause) of this irrationalism is that Sheldon overlooks the rational aspects of the experimental method in its most reliable form. For example, it is not the getting of intended results or of desirable practical consequences (whether intended or not) which constitutes the most dependable test of truth. It is rather the observing of what one had a *rational* right to expect to be able to observe, on the assumption that the hypothesis which guided experimental procedure is true. The *intellectual* noting of the correspondence between the *logically* reached prediction and the later observation, is what proves the hypothesis true; not the correspondence of the practical consequences with what was wanted or willed.

RAYMOND P. HAWES

SCIENCE AND THE IDEA OF GOD.
By William Ernest Hocking. The University of North Carolina Press, Chapel Hill, North Carolina. $1.50. lx + 124 pp. 1944.

FROM SCIENCE TO GOD.
By Karl Schmidt. Harper and Brothers. $2.00. x + 169 pp. 1944.

In recent years there has been held on the Atlantic seaboard an annual conference on science, philosophy, and religion. The reports of these conferences are generally out of print a few days after they are put on sale, a fact which indicates the widespread interest in the conferences on the part of the general public.

A generation ago any attempt to unify these three fields would have been called a reconciliation. Today the word used is integration, and the difference in meaning between these terms is an excellent index to the difference in attitude between nineteenth and twentieth century minds. A reconciliation involves a certain amount of concession and adjustment, based upon a mutual give and take. In the nineteenth century there were three outstanding reconcilers, one in each field—Le Conte in science, Fiske in philosophy, and Drummond in religion. All of these built on the foundation of Darwinian evolution, which from its origin in a specific field of biology has permeated all fields of thought and has had a unifying influence unparalleled in the history of human intelligence.

Today this synthesis has ceased to be a reconciliation, but it has not yet become an integration. We still tend to try to confine our scientific, philosophic, and religious beliefs in hermetically insulated compartments of our minds—an achievement possible only because we are all partly schizophrenic. It is interesting to speculate as to whether the present sorry state of civilization may not be due to the ubiquity of schizophrenia among those who determine international policies.

For such reasons as these any attempt to establish an integrated mind at the present time is not only appropriate but indeed most welcome. Schmidt's book, although small in bulk, is an ambitious attempt to establish theology on scientific basis. His argument is very ingenious, and runs somewhat as follows:

Your thoughts are not my thoughts, neither can I remember your experiences nor can you remember mine. The thoughts that bring you happiness may bring me sorrow, and vice versa. Your emotional responses may be very different from mine to the same stimulus. The only conclusion that we can draw from this is that your mind and my mind are distinct entities. And yet, you and I agree in our judgments in far too many instances for us to attribute the agreement to random coincidence. Without such agreement no science would be possible. Emmanuel Kant accounted for this agreement by postulating his *Ding an sich* and saying that when different minds contemplate the same object, they must necessarily come to the same conclusion. But since all of our knowledge of external objects comes through the senses, this is equivalent to defining that of which we have experience in terms of that of which we have no experience, which is no explanation at all. The only alternative is that there must be an Omniscient Intelligence who thinks these thoughts through our finite minds.

There are two implications contingent upon such

an assumption. Since it is inconceivable that such an Omniscient Intelligence could think an untruth, the criterion of the truth of a belief is the universality with which it is held. If it be true that the majority is always wrong at first, it is also true that eventually the truth will always prevail.

The other implication is that a universe without such an Omniscient Intelligence would be a chaotic universe. There could be no agreement among different minds, no common interpretation of external phenomena, no formulation of natural law, and therefore no science.

This implication is strangely inconsistent with the author's criticism of Isaac Newton, who postulated a first cause to start the universe going. The author states that there would then be no further need of such a Supreme Intelligence, inasmuch as natural law would suffice to keep the universe in motion. As a matter of fact, however, natural law is not a prime mover but a descriptive formula, and the fact that the processes of nature can be described by a formulated law is the best possible kind of evidence of the existence of a Supreme Intelligence.

Hocking's series of McNair lectures runs aground on the same reef. In his first lecture, he maintains that a universe so dependable that natural laws can be formulated to describe its past and to forecast its future cannot be ruled by a God who answers prayer. His remaining lectures are devoted to showing that only with such a universe is the existence of such a God compatible. The perusal of his book leaves the reader in a state of mental confusion—not so much as to what the reader should think or can think, but as to what Hocking really does think. Perhaps this was Hocking's object—to make the reader arrive at a conclusion for himself, without giving his own away.

A really scientific treatise on the interrelations of science, philosophy, and religion still remains to be written.

THE ROMANCE OF EXISTENCE.

By Ross Bundy. Pitman Publishing Corporation, New York. $2.00. vii + 188 pp. 1944.

This work is an example of what is technically called 'fine writing." What the author has to say is subordinated to the way in which he says it. Consequently the book must be judged as literature rather than as science. This does not mean that the author's knowledge of science is defective—it means merely that scientific knowledge has been demoted to a position in which it serves chiefly as a display case for the author's fluent verbal efflorescences.

The following sentence is illustrative of the literary style of the work: "And here one of the weirdest whims in the universe unfolded and flashed from its burning god—the sun god of that opulent splendor of the Nile and nearer Orient—5000 years ago—to insurge into the dead the light which produced life." Surely the writer does not mean to imply that the miracle of creation occurred only five thousand years ago. Even Bishop Ussher thought it was more remote than that. But what does he mean to imply? There are many other passages just as obscure.

The reader who is able to penetrate this literary style will find that on the whole the author's thought is lucid and logical, and that its apparent esotericity is the result of the complexity of his expression and his indifferent diction. To spell the plural of nautilus as nautilae is evidence of ignorance; to refer to the cycads as cyclads is evidence of carelessness; to speak of the vitalism of lower organisms when their vitality is meant is evidence of sloppy thought. The reviewer would have preferred greater accuracy and less embellishment. Are such words as necroptic, validant, ozaric, and mirrophonic necessary in what is obviously an attempt to popularize cosmic philosophy?

Yet there is much in the book to merit commendation. One needs only recall the author's definition of fossils as "silhouettes cut by death" or his comparison of the relation between mind and body with that between time and space. It is true that his speculations as to the light that relativity physics may throw on the doctrine of immortality are after all, only speculations, but they are reasonable speculations. And if his statement that matter is a far greater mystery than mind had been remembered by the philosophers of the past they might have avoided the morass in which psychology has for so long been mired, and from which it is only now with difficulty extricating itself.

This is not the sort of book that needs an index and the author has not bothered to supply one.

SCIENCE IN CHILDHOOD EDUCATION. *Practical Suggestions for Teaching, Number 8.*

By Gerald S. Craig. Bureau of Publications, Teachers College, Columbia University, New York. $1.00 (cloth); 60 cents (paper). vii + 86 pp. 1944.

It is encouraging to see that those responsible for the character of science teaching in the elementary and secondary schools are alert to its needs. Craig directs his remarks to teachers of the lower grades. He emphasizes the importance of seizing on the natural curiosity of children and the situations that arise spontaneously in play and daily life, for motivating scientific studies. He regards the inculcation of attitudes, particularly of the openmindedness and critical-mindedness of the scientific spirit and the recognition of the importance of planning, as more important than specific content. The curriculum itself should be planned, he thinks, around the development of desirable social attitudes and behavior. Sequence and continuity should be planned in terms of the child's own

growth and development. Basic principles to be taught should include: (1) the universe is very large; (2) the earth is very old; (3) changes occur continuously in the universe; (4) life is adapted to the environment; (5) living things are interdependent; (6) variation is manifested in both living and non-living things. While the child must learn to recognize that his community is but an interdependent part of a large world, community activities and resources may logically be made the center of organization of the curriculum. This subject is thoroughly illustrated with examples that should stimulate and aid the teacher.

Although somewhat wordy and repetitious, this booklet deserves a place on the shelf on every science teacher, particularly of those who teach on the elementary level.

BENTLEY GLASS

LIFE OVERFLOWS: *An Original Scrapbook Arrangement of Soothing Philosophical Thoughts.*
By Carl Leonard Thenebe. Bruce Humphries, Boston. $2.50. 116 pp. 1944.

A physician's moralizing in prose and verse, the quality of which is sufficiently apparent from the following:

"THINK WELL OF ALL
The rule of liking all, perhaps it even hurts.
Rewards will us befall, finesse will win converts!"

A NEW MANUAL FOR THE BIOLOGY LABORATORY.
By Bernard R. Weimer and Earl L. Core. John Wiley and Sons, New York; Chapman and Hall, London. $2.00 (paper). 213 pp. 1944.

This manual, written especially to accompany the textbooks of Strausbaugh and Weimer but designed for any elementary college course in general biology, includes approximately 50 pages of written directions and 150 pages of blanks for original drawings and outlines for labelling.

After introductory exercises with the microscope on plant and animal cells, including *Elodea* and *Ameba*, the structure of a typical angiosperm is presented. This is followed by directions for the study of food manufacture and storage in plants and then for the study in both plants and animals of digestion, food transfer, respiration, excretion, and irritability. The skeletons of the frog, bird, and man, and the muscles of the frog are considered next, followed by the reproduction of the flowering plants, the development of the frog, and, very briefly, homology. The section on heredity includes mitosis, meiosis in *Ascaris*, and simple *Drosophila* crosses. The directions end with two long sections, of necessity largely anatomical, one a survey of the plant, the other of the animal kingdom. Finally there is an exercise on ecology.

Both students and instructors will undoubtedly find this a very useful manual with many virtues. Structures are described with enough detail for the student to know what to look for. At the same time there are plenty of things left to the student, so that real demands are made on his own powers of observation. There is a reasonable degree of attention to the functional aspects of each subject, not only in the questions following each exercise, but also in the number of physiological experiments included, although the nerve-muscle section is disappointing in this respect.

There is a workable flexibility in organization which most instructors will want to utilize; e.g., it would seem desirable for students to work out the reproduction of the fern, which is given near the end of the manual in the section on the plant kingdom, before they attempt the reproduction of a flowering plant, which is given much earlier in the book. It seems strange to have the section on homology far separated from the section on vertebrate limbs, especially when it deals with limbs, albeit those of embryos. Various incidental matters need correction, e.g., pulvillus is used in place of tarsus, and fertilization is described as the uniting of "two large vesicular nuclei." The only constant confusion in using the manual arises from the purely typographical circumstance that technical terms and the numbers given to questions are printed in such very bold type that they completely overshadow the comparatively modest but much more important topical captions.

GAIRDNER MOMENT

ECOLOGY

ZEBU-CROSS CATTLE IN NORTHERN AUSTRALIA. *An Ecological Experiment. Bulletin No. 172.*
By R. B. Kelley. Council for Scientific and Industrial Research, Commonwealth of Australia, Melbourne. Free upon request. 96 pp.; 16 plates. 1943.

This photo-lithographed bulletin recounts the results of the introduction of Zebu genetic traits into the beef and dairy herds of northern Queensland. It will be of interest not only to animal breeders, but to geneticists and ecologists as well. After a preliminary discussion of the origins of modern cattle and the respective adaptive traits of European and tropical Zebu strains, the introduction of American-bred Zebus into Australia is described, together with the several hybridization experiments that followed. The results appear highly satisfactory. In the region of Queensland north of the tropic of Capricorn, the hybrids are much more resistant to ticks and heat than cattle of pure European extraction, and make better growth and produce an equal or superior type of beef on similar pasturage. The ideal intermixture appears to be

about three-eighths Zebu. The only drawback results from the higher intelligence and consequently lesser tractability of the hybrids. This is not serious. Although genetic data are inadequate for critical analysis, resistance to heat, to ticks, and to tick fever, as well as the other qualities mentioned, each appear to rest upon a multifactorial basis. Ecologically, it may be concluded that the Zebu type of cattle is genetically better adapted to tropical or subtropical habitats than European cattle are.

BENTLEY GLASS

CLIMATOLOGY.

By Bernhard Haurwitz and James M. Austin. McGraw-Hill Book Company, New York and London. $4.50. xi + 410 pp.; map and 17 plates. 1944.

This book was written to serve as an introductory text in climatology for meteorologists. It is not suitable for the general student. For students with a meteorological background, however, it is excellent.

There are two parts. The first, General Climatology, deals with the elements of climatology in a thorough way. Solar radiation, temperature, wind and pressure, air masses and fronts, cyclones and anticyclones, climatic classifications, climatic types, and microclimatology are discussed.

The second part describes the regional climatology of the world. In this section the climate of each area considered is described in terms of the meteorological elements. This gives the student the "why" of each climate and makes climates a matter of reason and understanding to an extent not usually found in climatology texts. This outlook deserves to be more widely applied.

There are ample maps, charts, and tables, and a rear pocket contains seventeen loose maps of climatological data. Students should find this a most useful text.

GEORGE F. CARTER

EVOLUTION

EVOLUTION, CREATION AND SCIENCE.

By Frank Lewis Marsh. Review and Herald Publishing Association, Washington. $2.25. 304 pp. 1944.

It is a matter of common knowledge that there are, not one, but many different theories of evolution. It is not so generally understood that there may be as many different theories of special creation. The theory expounded in this book will come as a surprise to those who are familiar with only that one based upon the postulate that there are as many species as there were originally created pairs.

Perhaps the greatest disservice ever rendered to the theory of special creation, according to the author, has been this dictum, which is of comparatively recent origin. The futility of attempting to generalize about species becomes evident as soon as one considers the various interpretations given this term by modern writers, most of whom feel constrained to distinguish between taxonomic and genetic species. To avoid this confusion the author has recourse to the Hebrew term *baramin* meaning "originally created thing," and as an example he mentions the varous breeds of dogs, which, although differing more widely than recognized species, are generally considered conspecific—an opinion in which the dogs themselves seem to concur. It is the baramin, and not any of the modern conceptions of the species which the author of Genesis had in mind when he wrote that every living thing reproduces "after his kind." Even today we speak of the cat kind, the horse kind, etc. These are all baramins, each descended from a single pair. The author of Genesis was not writing a scientific book, and to equate his terminology with that of modern science is quite unjustifiable. The author of the book under review is on solid ground when he insists that to render "kind" as "baramin" is to translate without interpreting, and so to preserve the meaning of the original text.

In rebuttal, it may be claimed that by uniting such diverse forms as the buffalo, the wisent, the aurochs, the bison, and the yak in a single baramin the author has accepted evolution in principle. But the theory that all forms of organic life have a common origin is a far cry from simple variation within each baramin. As an illustration of this difference the author discusses the many *Drosophila* investigations by modern students, with which he is thoroughly familiar. The number of mutations that may arise in a population of vinegar flies is legion, but they are never other than new forms of vinegar fly—no *Drosophila* mutant has ever been a dragon fly or a caddis fly or even a horse fly. And even the mutants that do appear are not strictly new, for they may be atavistic reversions to forms which have appeared many times in the ancestral stock. If we breed mutations long enough we can always get back eventually to the form from which we started. In other words we have variation without progression, and consequently without evolution. Here the author is again on solid ground.

Of course, the evolutionist, even though conceding that no new species has ever arisen as the result of a single mutation, can still claim that there is no reason why one should not arise from a concatenation of cumulative mutations. This theory is reasonable but it is not amenable to laboratory investigation because the human life span is too short. Even the elapsed time since the theory of evolution has been placed on a firm footing is too short for a relay of students to catch evolution "on the fly." The doctrine must be accepted on faith, because the universe as we know it is incom-

prehensible on any other basis. Doubtless the Omnipotent Creator could have set the cosmos going by a single divine fiat, if He had chosen to do so, but the obvious fact is that He did not so choose. The naive realist who believes only what his eyes can see or his hands can touch cannot accept the doctrine, and the materialistic philosopher who believes that the phenomena of life can be explained completely by the operation of physical and chemical laws cannot accept it consistently, although he may and often does accept it inconsistently.

The present reviewer is a confirmed evolutionist who believes the defense of special creation to be a lost cause, but he cannot dismiss this book as the fantasm of an irresponsible mind. Instead he can feel nothing but the greatest respect and admiration for Marsh, who is an acuitous and erudite student not only of biology but of the history of biological thought as well, and whose knowledge of practically all the modern work that has been done in genetics anyone might envy. The reviewer feels that the author has made a very telling point when he maintains that the fundamental cleavage plane in scientific thought is not between the evolutionists and the believers in special creation, for this plane of cleavage oscillates like a pendulum, and the belief that one generation may consider evolutionary may be thought creationistic by another. The unchanging cleavage, on the other hand, is that between the authoritarians and the experimenters, and this is true of other domains as well as of scientific thought.

The establishment of authoritarianism in science was due to the scholastic philosophers, who observed quite correctly that while contemporary scientists were in general agreement as to the fundamentals of their beliefs, the metaphysicians were tending to develop their own personal systems of thought. This seemed to indicate that philosophy was not keeping pace with science, and so they endeavored to divert the attention of original thinkers from science to philosophy, where it was felt that their talents were more needed. The result was that the scientific world through the dark ages to the time of Copernicus was dominated by the "ipse dixit" of Aristotle. In the meantime, philosophy, if not actually advancing, was at least expanding in complexity.

The authoritarians naturally tended to uphold the status quo, and innovations had to be based on rather convincing empirical evidence if they were to make their way against the accumulated beliefs of the ages. In the famous debate between Huxley and Wilberforce the latter's defense was almost exclusively authoritarian, for the theory of special creation had been in the saddle for over two centuries. Today the authoritarians are all evolutionists, and they prosecute their cause with the same vigor and the same arguments which their progenitors used on the other side three generations ago.

It was this dogmatic attitude, against which the Quaker and Huguenot blood of the author rebelled when he encountered it in his undergraduate days, that impelled the author to write this book. The present reviewer, who is of the same mixed ancestry, feels a great deal of sympathy for him. But the reviewer cannot share all the author's views, such as that the days of creation were of twenty-four hours each, for the sun whose rising and setting determine the length of the day was not created until the fourth day. Neither can he concur in the statement that there is no inconsistency between the Elohistic and Jahwistic narratives in Genesis, for the former declares man and woman coeval, while the latter speaks of the woman's costal rib origin. But he does believe it would be well if all theorists could write with the same tolerance and respect for those with whose views they do not agree, and with the same straightforward clarity of expression which contrasts so strongly with the ponderosity of verbiage and concomitant obfuscation of thought which philosophers still delight in.

The lack of a detailed index is the most serious defect of this book.

A Story Outline of Evolution.
By Charles W. Grimes. Bruce Humphries, Boston. $2.00. viii + 244 pp. 1944.

Appearing under the imprint of a new publisher and ostensibly revised, this is essentially the same book previously reviewed in this journal (vol. 13: 79. 1938), and the remarks then made may stand without essential alteration. The biological portion of the book is not up to date, and errors such as the identification of the notochord with the nerve cord are numerous. It should also be pointed out that the entire concept of evolution developed here is teleological. God (Nature, or purpose) intervenes in each and every step, not as prime cause but as immediate agent. It is unfortunate that the author's worthy ambition to show laymen how evolution and religious thought are in harmony has not been accompanied by a sounder knowledge of his subject.

Bibliography of Fossil Vertebrates 1934–1938. *Geological Society of America Special Papers, Number 42.*
By C. L. Camp, D. N. Taylor, and S. P. Welles. Geological Society of America, New York. $4.75 (paper). v + 663 pp. 1942.

This volume embodies the second part of the stupendous task of bringing together the literature of the world pertaining to fossil vertebrates. Camp and his associates included in the first volume papers published in the period 1928–1933, and the present one covers the period from 1934–1938. The bibliography is in

three parts which cross-index one another: 1) a catalogue of papers by alphabetical arrangement of their authors; 2) a subject index; and 3) a systematic index. Appended is a list of the serial publications used, supplementary to that given in the first volume; and also a synoptic classification. These two volumes are a continuation and extension of the work of O. P. Hay, who published in 1902 and in 1930 bibliographies of the papers dealing with the fossil vertebrates of North America up through 1928. These, together with the yet unpublished survey by A. S. Romer of the literature of the world (exclusive of North America) prior to 1900, will give to vertebrate paleontologists an exceedingly useful coverage of their field.

MIOCENE FISHES OF SOUTHERN CALIFORNIA. *Geological Society of America Special Papers, Number 43.* *By Lore Rose David. Geological Society of America, New York.* $1.70 (paper). xiii + 193 pp.; 16 plates. 1943.

The specimens described in this paper represent 39 families, and were collected from the eastern and northern parts of the Santa Monica Mountains, from the Palos Verdes Hills of Los Angeles County, and from the Santa Maria Basin. Fourteen new species and five new genera are described.

Parts I to III of the paper describe the fossil fish fauna from different horizons of the Los Angeles Basin. Part I: Collections of fossils were made at a number of localities on the west side of Sepulveda Canyon, Santa Monica Mountains. The fishes occur in hard siliceous sandstone and shale of the lower Modelo formation (Upper Miocene). Eighteen families and twenty genera are represented. The assemblage suggests a marine neritic subtropical fauna, living at a depth of 100 to 200 meters. Part II: The diatomaceous shale of the Upper Modelo formation was collected at localities near Van Nuys along the northern rim of the Santa Monica Mountains. The restricted fauna of a bathypelagical environment is represented by nine genera. Part III: Fossil fish are described from marine deposits of late middle to early Upper Miocene age (Altamira shale) on the coast of the Palos Verdes Hills, near San Pedro. A subtropical shallow sea is indicated at the place of deposition of this particular assemblance. Part IV: The herring-like fishes of the California Miocene are reviewed. This type of fish is the most common in the Miocene deposits. Part V: A study of the extinct family Zaphlegidae, which was related to the Gempylidae and lived in the offshore deeper water. The reasons for its extinction were possibly climatic changes toward the end of the Miocene. Part VI: A complete revision of the fish fauna from the diatomaceous earth of Lompoc, Santa Barbara County, California. The fauna comprises 31 families, 39 genera, and 42 species. It represents a harmonious assemblage that accumulated in a warm-temperate coastal sea of 25 to 50 fathoms depth, half a mile offshore. The author presents evidence that a greater similarity existed between the fish faunas of the Pacific and Atlantic coasts of America during the Miocene than at present. A relatively great number of forms has become extinct in the California sea since Miocene times.

HADROSAURIAN DINOSAURS OF NORTH AMERICA. *Geological Society of America Special Papers, Number 40.* *By Richard S. Lull and Nelda E. Wright. Geological Society of America, New York.* $2.25 (paper). xi + 242 pp.; 31 plates. 1942.

The present paper is a monographic review of a group of dinosaurs which has been extensively studied, as is emphasized by the 194 titles in the bibliography. The value of a uniform approach to such a complex and interesting group of animals is obvious. The text includes the geological and geographical distribution of the hadrosaurs, and a discussion of the climate and the plant and animal environment, as gathered from the structure and fossils of the associated rocks. The major portion of the monograph is devoted to a discussion of the morphology, and a systematic description of genera and species. The skeleton is described in great detail, and with excellent illustrations. Brain casts of one species are figured, and an attempt is made to reconstruct the muscular system. The shape and size of the muscles of fossil forms are deduced from the structure of the bones to which they are attached. It is well known, of course, from a study of living vertebrates, that the presence or absence of bony ridges, fossae, spines and tubercles may often be very misleading when compared with the associated muscles. Such a reconstruction as that made in the present volume is valuable, but should be regarded in only the most general sense. The family Hadrosauridae includes, in this monograph, 23 genera (of which seven are of doubtful status) and 52 species.

ORIGIN OF MAMMALS BASED UPON CRANIAL MORPHOLOGY OF THE THERAPSID SUBORDERS. *Geological Society of America Special Papers, Number 55.* *By Everett C. Olson. Geological Society of America, New York.* $1.00 (paper). xi + 136 pp. 1944.

This excellent anatomical study of mammal-like reptiles is based upon serial section and reconstruction of the skulls of representatives of the suborders Gorgonopsia, Therocephalia, Anomodontia, and Cynodontia. A detailed description is given for the inner ear, middle ear, the occipital region, and the basicranial axis. Brain casts, and certain parts of the jaws, face, and calvarium are discussed. The author subscribes

to a polyphyletic hypothesis of the origin of mammals. It was suggested by Romer (1940) that the therapsid reptiles stemmed from the suborder Sphenacodontia (order Pelycosauria) in the lower Permian. Olson has given additional evidence that the early therapsids developed into many lines in the Permian, each of which attained independently varying degrees of "mammalness." In other words, "each line was approaching the mammalian threshold by developing characters to be found in mammals, but developing these characters in various relationships and in different sequences temporally." The end of the Permian, over much of the world, was marked by a development of aquatic conditions, and the therapsid groups were impoverished. Four of the lines surviving the Permian-Triassic transition were potential ancestors of the primitive mammals. From these four groups (cynodonts, bauriamorphs, ictidosaurs, protodonts), and perhaps also from other similar but unknown lines, mammals arose in at least three distinct phyla (monotremes; multituberculates; and a group including the pantotheres, symmetrodonts, triconodonts, and marsupials). It remains to determine the therapsid source of each of the mammalian groups, for only vague generalities are at present possible. The results of this very significant paper have added materially to our understanding of the origin of mammals.

THE GEOLOGY AND PALEONTOLOGY OF THE MARINE PLIOCENE OF SAN DIEGO, CALIFORNIA. PART I. GEOLOGY. *Memoirs of the San Diego Society of Natural History, Volume II.*

By Leo George Hertlein and U. S. Grant, IV. San Diego Society of Natural History, San Diego. $1.50 (paper). 72 pp.; 1 map + 18 plates. 1944.

This monograph is introductory to the forthcoming description of the marine Pliocene invertebrate fauna of the San Diego region. It describes the physiography and stratigraphy of the region, and is admirably illustrated and provided with maps. Lists of species are given for the Cretaceous, Pliocene, and Pleistocene deposits of the region.

GENERAL AND SYSTEMATIC BOTANY

AMERICAN BOTANY 1873–1892: *Decades of Transition. By Andrew Denny Rodgers III. Princeton University Press, Princeton.* $3.75. ix + 340 pp. 1944.

This volume is comprehensive in the wealth of material it presents, discriminating in its selection of significant and revealing correspondence, and searching in its interpretations of the inter-relation of trends and activities in the rapidly growing field of botany.

Centering around Asa Gray as leader and pioneer, the book traces the development of a richly productive period of exploration, collection, and monographic taxonomy, with due attention to Gray's associates and to the work of the younger men stimulated by them to enthusiastic activity. Although complete in itself, the volume is most effective when read in connection with the companion biographies of Torrey, Sullivant, and Coulter, for the lives and work of the many botanists involved run from one to the other in a closely interwoven relationship. Extending out from the activities of Gray and his associates, the development of related fields is traced; the growth of paleo-botany from the pioneer work of Lesquereux; the stimulating influence of Darwin's interpretation of evolution, strengthened in its effect in this country by the staunch support of Gray; the development of botanic gardens for experimentation and research rather than for display; the rapid advance of state experiment stations and the influence of Bailey and others in gaining recognition for horticulture and other applied aspects as worthy branches of botany; the rise of the scientific laboratory approach correlated with observations and collections in the field, and the resulting advance of some of the more academic scientific phases of botany.

As in the author's other books, this is no mere chronicle of events and contributions, for the personalities of the many botanists are made vivid and living by illuminating excerpts from correspondence, giving strong support to Emil Ludwig's belief that "it is the anecdote that best reveals the personality." The chapter on the controversy between Gray and Greene significantly reveals the personalities not only of these two men, one representative of the botanical activity of the more conservative East coast, the other of the more radical West, but also of the other botanists associated with them.

If there is any criticism that may be advanced by the reader, it might be that throughout this book and its companion biographies, the interweaving of activities and influences of the many personalities and groups involved become so complicated that the reader inevitably is somewhat bewildered, yet the ample and detailed index facilitates rapid reference to any one botanist or any phase of the complicated picture, offering helpful guidance through the maze of activities, personalities, and events.

That the younger botanists of the day are more inclined to think in terms of the present and of the future is wholly natural, but reading this book and its companion volumes will bring to them the realization that they can profit greatly by these interpretive surveys of the botanical past that is their heritage. To play their part in the development of botany in the United States was the destiny of the many pioneers whose work and personalities are so ably presented in these books; to grasp, interpret, and present effec-

tively with appreciation and understanding the contributions of these men is in itself a contribution to the science, and it would be fitting for some botanical society to make Rodgers an honorary member, since in him botany has found an interpreter, spokesman, and historian.

WM. H. WESTON

AQUATIC PLANTS OF THE UNITED STATES. *Handbooks of American Natural History Volume IV. By Walter Conrad Muenscher. Comstock Publishing Company, Ithaca, N. Y.* $5.00. x + 374 pp. 1944.

This book invites comparison with Fassett's *Manual of Aquatic Plants*, and the comparison reveals considerable differences. Although both books are profusely illustrated, so that plants can often be identified by means of the drawings, the accompanying keys are widely divergent. Fassett's key is avowedly designed for the identification of sterile plants, while the extensive use of differences in reproductive forms by Muenscher makes it impossible in many cases to identify a plant by its vegetative characters alone.

The scope of the two books also differs, in that Fassett confines himself to aquatic plants of the eastern United States, but includes all the macroscopically visible plants excepting the fungi; and Muenscher, while covering the whole of the United States, includes only Pteridophytes and Spermatophytes.

A further difference is in the classification of forms and varieties, in regard to which Fassett is a "splitter," and Muenscher a "lumper," although the latter does describe the variation within species.

Muenscher's key and his distribution map for each species are very practical, and the drawings are excellent. The book is a welcome addition to this field.

EILEEN SUTTON GERSH

THE AQUATIC OOMYCETES OF WISCONSIN. *Part One. By Frederick Taylor Wolf. The University of Wisconsin Press, Madison.* $1.50. 64 pp. 1944.

Under the above title, the author has collected and organized, in brief monographic form, the results of his own and others' extensive investigations of the aquatic fungi of Wisconsin. The book contains detailed descriptions of fifty-four species, ranging through six orders and twenty-two genera of aquatic oomycetes, records of their collections in the state, and keys for their identification, as background for further and more intensive work on these problematical organisms. Several plates of original drawings, and a very complete citation and summary of previous publications on the aquatic Phycomycetes accompany the work.

L. M. LANGDON

ROOT DISEASE FUNGI. *A Treatise on the Epidemiology of Soil-borne Disease in Crop Plants, and a First Exposition of the Principles of Root Disease Control. Annales Cryptogamici et Phytopathologici, Volume I. By S. D. Garrett. The Chronica Botanica Company, Waltham, Massachusetts; G. E. Stechert and Company, New York.* $4.50. 177 pp.; 1 plate. 1944.

Preeminently qualified by his widely known investigations of various root diseases in Australia and Great Britain, the author here presents a volume highly valuable and significant. Although primarily concerned with the fungi attacking the root systems of crop plants, the book gives adequate recognition and careful consideration to the part played by the physiological condition of the plant and by the nature of the living and non-living components of the soil.

The introduction covers the historical background of the subject, tracing the shift in emphasis from the early preoccupation with the parasite itself to the more recent realization that such factors as temperature, acidity, and the microbiological equilibrium of the soil strongly influence the incidence and severity of root diseases, and discussing the application of this broader knowledge to their control.

Five detailed chapters discuss the parasitic activity of the root-infecting fungi, covering their specialization, methods of spread, and the influence of the soil upon them through such diverse features as temperature, moisture, texture, reaction, organic content, and concentration of nutrients.

The saprophytic activities of these fungi in spreading through the soil or in persisting in dead tissues, and their dormant survival in the soil by means of resting spores or sclerotia, are adequately covered in Chapters 7 and 8.

The remainder of the book, about one-half of its total, is devoted to a comprehensive and thorough discussion of the control of root diseases from the ecological point of view. In ecological categories of crop conditions, control is considered first in field crops by such measures as rotation, plant sanitation, and modification of the soil under the growing crop; then in plantation crops on virgin areas or on areas long planted, with also a brief discussion of special problems presented by certain types of diseases; and finally, in glasshouse crops, where more direct attack by partial sterilization of the soil is facilitated by the limited area of soil enclosed under more regulated conditions.

The nine figures, ranging from illustrations of fungus mycelium in the root tissue to maps of the spread of root diseases under different conditions of cultivation,

so helpfully supplement appropriate parts of the text that one realizes the value of the book could have been greatly enhanced by more such illustrations.

A bibliography comprising nearly 450 titles ranging from the early classics, such as Woronin's 1878 paper on club root of cabbage and Thaxter's publications in 1891–92 on potato scab, to the most recent literature gives an extensive background and commands admiration for the searching work of the author in so thoroughly covering the extensive field. A general index of nine pages and an author's index of four handily expedite reference to the many phases covered in the volume.

As the first volume of *Annales Cryptogamici et Phytopathologici* the book auspiciously begins this new series, admirably maintaining in its field the high standards already established under the editorship of Verdoorn in the fourteen volumes of the companion series of plant science books previously reviewed here.

WM. H. WESTON

PLANT MORPHOGENESIS

ORGANOGENESIS IN RUBUS. *University of Hawaii Research Publication Number 21.*

By Charles J. Engard. University of Hawaii, Honolulu. $3.75 (paper). xvi + 234 pp. 1944.

The reader should not be misled by this title, for the scope of this bulletin is far more extensive than might be assumed. The writer not only has made a thorough histogenetic and morphogenetic study of an entire plant but throughout the work has considered the plant as a unit, all parts of which are physiologically correlated. Motivated originally by an interest in sieve-tube structure derived from his previous studies on solute translocation in *Rubus*, the writer has followed this investigation for more than six years. The four species of *Rubus* that are the subjects of this intensive study were chosen because they are representatives of widely separated and distinctive natural localities and represent the two sub-genera recognized in this large and varied genus.

After considering such fundamental matters as the classification and gross morphology of the genus and the methods used in carrying on the work, the main body of the book comprises the detailed and thorough study of the stem apex, of the foliage leaf, of the stem, comprising vascular and other tissue systems and cells, of the root, and of the flower.

Using the approaches of the modern school of morphologists, the writer here submits an admirably thorough study of the origin and development of all tissues, organs, and appendages of the four subject species, correlating the ontogeny with physiological relationships to the organism as a whole.

Any botanist, whether or not he is of the elect who will follow with absorbed interest the wealth of technical data in the body of the paper, will profit by a careful reading of Chapter XI. This chapter, under the title "Organa Sui Generis or Organa Homologata," develops further the concept of morphology upon which the writer bases his whole study. His analysis of Goethe's conception and interpretations is the most adequate presentation that has appeared in any recent morphological treatise. His reconsideration of Goethe's theories and concepts in the light of modern research is most illuminating. His reconciliation of some of the conflicting interpretations of other morphologists shows notable breadth and vision. All in all, the scope of this volume extends far beyond an histological study of four species of one genus, its implications and bearing making it an important contribution that undoubtedly will stimulate further work in this fundamental field.

WM. H. WESTON

PLANT NUTRITION

THE DIAGNOSIS OF MINERAL DEFICIENCIES IN PLANTS BY VISUAL SYMPTOMS. *A Colour Atlas and Guide. Supplement 1944.*

By T. Wallace. His Majesty's Stationery Office, London. 5s. 48 pp. 1944.

This supplement to the excellent color atlas previously reviewed (vol. 19: 341. 1944) adds 95 color plates showing the effects of deficiencies on additional crops (flax, pea, red clover, lucerne, lettuce, radish, leek, celery, pear, plum, cherry) and additional deficiencies, in particular nitrogen deficiencies, for crops previously included. An index to the combined original edition and supplement has been prepared. The original edition and supplement may now be obtained bound as one volume (15s.).

SOIL AND PLANT ANALYSIS. *A Laboratory Manual of Methods for the Examination of Soils and the Determination of the Inorganic Constituents of Plants.*

By C. S. Piper. Interscience Publishers, Inc., New York. $4.50. xiv + 368 pp. 1944.

This authorized photo offset, reprinted from the original published by the University of Adelaide, Australia, in 1942, greatly extends the sphere of usefulness of this handy volume. A highly practical book designed to serve as a technical laboratory manual, it will prove directly useful to agricultural chemists and others whose field of work touches either pedology or plant analysis, since the book comprises the methods in actual use in the laboratories at Waite Institute, where the author has worked as chemist for nearly twenty years.

Part I, comprising about two-thirds of the book, is concerned with methods for the examination of soils, and follows in detail all of the steps involved from the collection of the soil samples in the field and their preparation in the laboratory through the many types of precise and exacting analyses. There are detailed discussions of suitable procedures for the determination of hydrogen-ion concentrations and of conductivity; methods of mechanical analysis of water-holding capacity; the determination of calcium carbonate, analysis of the hydrochloric acid extract, and determination of the exchangeable ions. Determination of nitrogen, nitrates, nitrites, and ammonia as well as of organic matter is well treated, and the separation of the clay fraction, with the determination of silica, iron, and alumina, is adequately covered.

Part II, in approximately 100 pages, presents the methods for the determination of the inorganic constituents of plants, covering the preliminary steps such as the collection and preparation of samples and the various approved methods for ashing, and the precise procedure for the determination not only of the commoner inorganic constituents but also of the trace elements.

The illustrations, in 19 line cuts, helpfully depict some of the essential apparatus used, giving details of assembly and construction. Pertinent references follow each chapter so that no assembled bibliography is necessary. Adequate subject indices, one for soil analysis and another for plant analysis, complete the book.

A thoroughly practical and usable manual, the book will prove a valuable tool in its technical field, a field in which relatively few such comprehensive guides are now available.

WM. H. WESTON

ECONOMIC BOTANY

THE PEATS OF NEW JERSEY AND THEIR UTILIZATION. *Part B of The Peat Resources of New Jersey. Bulletin 55—Part B, Geologic Series, Department of Conservation and Development, State of New Jersey.*

By Selman A. Waksman, H. Schulhoff, C. A. Hickman, T. C. Cordon, and S. C. Stevens. Department of Conservation and Development, State of New Jersey, in cooperation with New Jersey Agricultural Experiment Station, Rutgers University; Trenton. $1.00 (paper). 278 pp. 1943.

This report on the quantity, availability, and quality of the peat deposits of New Jersey was prepared for the purpose of aiding the exploitation of these deposits as a valuable natural resource which had become increasingly important in the state for replacing the peat formerly imported from the Scandinavian countries and Germany. Since about 70 per cent of the peat areas in the state were carefully surveyed, and this percentage was representative in nature and distribution, adequate information was secured for generalizations as to the total resources. The distribution of these deposits and their area, depth, and availability for exploitation are considered, as well as the quality and the botanical and chemical nature of peat itself. The thoroughness and comprehensiveness of the survey is apparent even from the introductory presentation of the plan followed and the methods used.

Chapter I covers fundamental matters involved in the geography, geology, and topography of the peat lands. These are not inconsiderable, since they occupy about 20 per cent of the undeveloped land of the state. Especially interesting to the biologist is the discussion of the vegetation factors involved in peat formation, with pollen analyses of representative bogs and studies of the associated calcareous marl formations.

Chapter II covers the properties, nature, and distribution of the peats in their more general aspects through the 21 regions into which, for the purposes of this survey, the state was divided, according to drainage basins and the bog, swamp, and marsh areas.

The body of the bulletin, comprising 158 pages, presents in three successive chapters detailed information on the three regional types of peat deposits: the northern, predominantly of the reed and sedge type; the coastal plain, predominantly of the forest type; and the salt marsh and alluvial types. The material in these three chapters, representing the carefully studied data accumulated in more than three years' work, is detailed and comprehensive in itself and is amplified by a wealth of illustrations in 15 plates and 42 figures comprising maps, cross-section diagrams, and photographs of ecologic associations.

The final chapter, on the utilization of peats in New Jersey, surveys the use of tidal marshes reclaimed for agriculture by diking and draining for pasture and hay or for growing cranberries, blueberries, or certain suitable truck crops; the "mining" of buried cedar logs still profitably carried on after nearly 200 years of exploitation; and the direct uses of peat itself for such purposes as the improvement of soils. It is interesting to note that the peat is almost all of the sedge and reed type or of the forest litter deposit type, since moss peat, resulting from long accumulation of sphagnum in quantities suitable for large scale development, is practically lacking in the state, although some surface sphagnum in the bogs is available and has been exploited on a small scale. The availability and use of so-called "orchid" peat, which consists of the fibrous roots of the osmunda or cinnamon fern, is touched on briefly, while as an example of the use of leaf mold the production at one nursery is reported as having increased from 10 tons in 1939 to over 600 tons in 1941.

A bibliography of 110 titles, containing pertinent publications from 1765 to the present, and an adequate subject index of four pages complete the book.

After appraising the possibilities for peat utilization outlined in chapter VI and its potential value to the state, the reader cannot but wonder whether this project #65-1-22-477 of the WPA—lasting over three years and employing at one time a staff of over 150 persons, comprising three state and four district supervisors, 15 to 25 field crews of five men each, an office staff of clerks, draftsmen, typists, and an artist; a laboratory staff of chemists, assistants, and laborers; and a production staff of authors, experts, collaborators, and statisticians,—accomplished results that will yield the state a return commensurate with the expenditure. There is no doubt, however, that this bulletin should stimulate an influx of botanists and seekers of the unusual, for, quite in addition to the rich and interesting collecting grounds described, such alluring names as Buckshutum Swamp, Great Egg Harbor, Jenny Jump Mt., Ongs Hat, Pimple Hills, Assanpink region, Pochuck Creek, Kittatinny, Pocanoe, Tuckahoe, Allamuchy, and Wawayanda will make even the least impressionable reader wish to fill up the old car with gas and start on extensive exploring trips as he might have in the luxurious days when this survey was made.

WM. H. WESTON

FORESTRY ON PRIVATE LANDS IN THE UNITED STATES. *Duke University School of Forestry Bulletin 8.*

By Clarence F. Korstian. Duke University, Durham, N. C. $1.50 (cloth); $1.00 (paper). xiii + 234 pp.; 20 plates. 1944.

The status, problems, and prospects of private forestry in the United States, as revealed in an extensive and impartial survey of eight important timber states of this country, are discussed and summarized in this report in a consideration of measures essential to the application of better forestry practices to privately owned forest properties. Adequate Control of Forest Fires, Extension Forestry, Farm Forest Cooperatives, Forest Lending Agencies, Cooperative Research, Forest Taxation, and Public Control of Cutting Practices are some of the important subjects dealt with by an authority on these questions; as also legislative and administrative measures necessary to insure constructive forestry programs on private lands. An Appendix with a section on "Forest Terminology," and a carefully prepared bibliography of over 150 titles constitute a further valuable contribution.

L. M. LANGDON

COMMERCIAL FLOWER FORCING. *The Fundamentals and Their Practical Application to the Culture of Greenhouse Crops. Fourth Edition.*

By Alex Laurie and D. C. Kiplinger. The Blakiston Company, Philadelphia. $4.50. viii + 598 pp. 1944.

While this is a practical manual for the florist and greenhouse man, it contains an abundance of information that will be most useful to professional botanists. The new edition includes a chapter on gravel culture, and desirable nutrient levels for greenhouse crops are given in tabular form. Soil sterilization, fertilizers, temperature control, the importance of humidity, use of growth promoting and root-forming substances, methods of propagation, and new measures of pest control are but a few of the valuable topics considered. The chapter on greenhouse construction and heating should be very useful to departments considering new construction. The section dealing with individual crops will also be of value. The book is to be recommended as a standard manual for all botany and biology departments equipped with greenhouses, and to all flower growers, whether for profit or pleasure.

GENERAL AND SYSTEMATIC ZOOLOGY

AN INTRODUCTION TO ANIMAL BIOLOGY. *Second Edition.*

By John B. Parker and John J. Clarke. The C. V. Mosby Company, St. Louis. $3.75. 532 pp. 1945.

The second edition of this textbook on animal biology has been revised and brought up to date, although the essential contents and manner of presentation have not been altered. The authors have written the subject to make it serve as a means to teach the student, and to illustrate and correlate the information garnered in the lecture room and the laboratory, and not as a reference text loaded with data not essential to the general biology student. With this view in mind, the volume has indeed been well written and should serve the purpose for which it is intended. Many of the illustrations are original, which is a fact always to be admired in college zoology texts. There is a suitable index and a good glossary.

HENRI C. SEIBERT

ATLAS IN GENERAL ZOOLOGY.

By Alfred M. Elliott. Published by the author, State Teachers College, Bemidji, Minnesota. $1.20 (unbound). 62 plates. 1944.

This envelope of loose-leaf drawings has been prepared by the author to appeal more widely than do standard illustrations to those students whose major interest lies outside the field of biology. He has certainly succeeded in being graphic and interesting. The diagrams are outstanding in clarity, details having been sacrificed for principles. Three-dimensional effects are used, humor is skilfully and appropriately intro-

duced, and the order of artistry is high. The style of illustration is very reminiscent of that used in Buchsbaum's *Animals Without Backbones*, and the similarity in some instances raises a wonder as to why acknowledgment has not been made.

Only a single doubt intrudes in the reviewer's mind. Will the student who is given these plates tend to rely upon them overmuch? Will there not be a temptation to make them a substitute for the accurate, painstaking observation of objects in the laboratory, and the effort to delineate them, that together are so important in teaching the methods and spirit of scientific inquiry? If this danger can be avoided, the atlas should be a very worth-while teaching aid.

BENTLEY GLASS

INTRODUCTION TO PARASITOLOGY.

By A. S. Pearse. Charles C. Thomas, Springfield, Illinois; Baltimore. $3.75. ix + 357 pp. 1942.

A tremendous amount of information is condensed into this little volume. It represents a survey of the entire animal kingdom for parasitic organisms. It is not intended to be an exhaustive presentation, but the forms discussed are certainly numerous and varied. Although the book is broad in its scope, it does stress the parasites of man. For instance, entire chapters are devoted to malaria and hookworm, inasmuch as they are particularly important in the United States. However, most of the human forms are treated rather superficially and, sometimes, none too accurately. Despite the emphasis on the human forms, the greatest majority of the organisms discussed never attack man. Nevertheless, many of them do not lack human interest, since they are economically important because of their attacks upon wild and domestic animals that man uses as food.

Pearse has planned the book to furnish reference material to supplement lectures and laboratory work, and not as a textbook adapted to reading assignments. As he has expressed it, "it is hoped that it will be useful in 'looking things up,' whether it is used in a class room, on a farm or a fishing boat, in a packing house or a seaside laboratory." It will no doubt find a place on the reference lists of courses in general biology, invertebrate zoology, and ecology, as well as on those in introductory parasitology.

Due to the reference nature of the book, much of it is not very readable. However, scattered among the concise morphological descriptions are fascinating bits of information on parasitism—the fish that deposits her eggs inside of clams; ants that obtain most of their food from the tongues of other ants that are their slaves; the minute fish that swims into the urethra of man; birds that lay their eggs in nests of other species; and countless others. Other interesting portions of the book concern the evolution of the parasitic habits of the various organisms.

The text material is profusely illustrated with clear-cut line drawings. The reviewer is by no means acquainted with all of the forms pictured, but the poor illustrations of some of the human parasites makes him wonder if the figures of the non-human forms are adequate for correct identification. The drawing of the malarial parasites (fig. 77) is exceptionally poor.

One chapter is devoted to the methods of examining fecal samples for intestinal parasites. The different procedures of concentrating and enumerating such organisms are discussed briefly. In this discussion the author expresses the attitude of the "true" parasitologist, which is sometimes difficult for others to appreciate. "The examination of feces day after day might be thought to be tiresome routine, and it is certainly malodorous!; but when it is to furnish data which may be used to change the lives of whole communities and nations, it becomes interesting and exciting. For a parasitologist the discovery of a worm egg, an amoeba cyst, or a malarial parasite gives more pleasure and satisfaction than a fisherman gets from catching a fish or a hunter from shooting a duck. It is real sport!" More technique is presented in the last chapter. The author summarizes methods of collecting, preserving, staining, and mounting parasitological specimens. It is divided according to the major taxonomic groups in order to emphasize the techniques particularly applicable to specialized structures.

M. M. BROOKE

DEVESCOVINID FLAGELLATES OF TERMITES. IV. THE GENERA METADEVESCOVINA AND PSEUDODEVESCOVINA. *University of California Publications in Zoology, Volume 45, No. 4.*

By Harold Kirby. University of California Press, Berkeley and Los Angeles. $1.25 (paper). Pp. 247–318; plates 32–44. 1945.

In this fourth in the series of monographic treatments of devescovinid flagellates, sixteen species of *Metadevescovina*, thirteen of them new, and four species of *Pseudodevescovina*, none new, are described. Drawings are given of each species. Particularly interesting are the varied forms taken by the axostyle and parabasal body. All the species of *Metadevescovina* except one are confined to a single family of the termite host. *Pseudodevescovina* is reported only from the genus *Neotermes* in Australia and Africa.

HANDBOOK OF THE MOSQUITOES OF NORTH AMERICA. *Their Anatomy and Biology; how they can be studied and how identified; how they carry disease and how they can*

be controlled. Second Edition: Revised and Amplified. By Robert Matheson. Comstock Publishing Company, Ithaca. $4.00. viii + 314 pp. 1944.

For untold centuries, mosquitoes, with their insidious desire for fresh blood, have plagued man and beast to such an extent that in some areas coexistence has been made unbearable if not impossible. Although they still rate as notorious pests, the great attention that is now focused upon them is primarily due to their ability to transmit certain diseases. In North America, there are four important human diseases that are known to be transmitted only by mosquitoes—malaria, dengue, yellow fever, and filariasis. These same diseases are even more prevalent in many parts of the world where our service men are now stationed. Undoubtedly, numbers of them will acquire infections and return to this country, where the stage is already set for transmission. Furthermore, exotic strains of these diseases may be introduced which might intensify the situation. It is therefore, particularly important and timely that Matheson should present a revised edition of his valuable handbook. It is his hope that by making authoritative information readily available, those concerned with the public health of our country will be able to correctly identify the medically important mosquitoes and thus be in a position to institute more effective methods of control.

The present edition is organized on the same pattern as the first. There are two major divisions; the first part deals with the more general aspects of the subject, and the more extensive second part presents a systematic account of the North American mosquitoes. Many of the chapters have the same titles and subheadings, but inasmuch as the first edition appeared over fifteen years ago, much revising and amplifying has been necessary. The following are a few of the changes and additions that appear in the revised edition. Thirteen species of *Anopheles* are presented instead of eight; *freeborni, bradleyi, georgianus, ablimanus,* and *franciscanus* having been added. Within the tribe Culicini, *Mansonia* is now presented as a separate genus, *Taeniorhynchus* is considered as a synonym of *Aedes, Culiseta* has gained priority over the term *Theobaldia,* and *Megarhinus* has been removed from the Culicini and placed in a separate tribe, Megarhinini. Whereas originally the Chaoborinae were barely mentioned, a new chapter is now devoted to this subfamily of the Culicidae. New keys to the genera and species have been written, and nearly twice as many plates and figures illustrate the text material.

By means of intelligible keys, the author leads the reader from the family Culicidae of the order Diptera down through the taxonomic subdivisions to the various genera and species. In many instances, keys are given for three stages of the mosquitoes, larvae (4th instar), pupae, and adults. No attention is given to the characteristics of the eggs and first instar larvae. For the adult stage, the characteristics of the male genitalia are extensively used. The author also presents brief discussions of each of the species found in the keys. In these discussions, the synonyms are mentioned, and references are made to the literature containing the original descriptions. For the important species, the discussions present the diagnostic characteristics of the various stages, their known distribution in North America, and information on their biological habits.

An index is presented, as well as an extensive list of references. These references are conveniently divided into "general works," "mainly biological," "mainly taxonomic," and "mosquito control."

M. M. BROOKE

A REVIEW OF STUDIES ON THE MEXICAN FRUITFLY AND RELATED MEXICAN SPECIES. *United States Department of Agriculture Miscellaneous Publication No. 531. By A. C. Baker, W. E. Stone, C. C. Plummer, and M. McPhail. U. S. Government Printing Office, Washington.* 35 cents (paper). 155 pp.; 10 plates. 1944.

This publication summarizes the results of studies conducted in Mexico by the laboratory of the Bureau of Entomology and Plant Quarantine of the United States Department of Agriculture, located in Mexico City. The fruitfly *Anastrepha ludens,* is a native insect of Mexico, attacking chiefly the citrus and the mango, which were introduced many years ago. The most commonly favored native host is the yellow chapote (*Sargentia greggii*). The results obtained and the conclusions derived from laboratory experiments, from field investigations, and from published material are presented in detail. The economic aspects, such as the disposal of infested fruit, prevention of infestation, poison sprays, methods of trapping and counting populations, are naturally stressed. No definite conclusions with regard to control have apparently been reached.

There are other fruitflies in Mexico, and their identification in some instances is difficult. The shape of the ovipositor has been found useful as a diagnostic character, as well as various anatomical structures in the larvae. It is interesting to note that in many instances, differences in biological habits have later been associated with forms that have proved to be taxonomically distinct on careful reappraisal of their external morphology.

HENRI C. SEIBERT

THE MOLLUSCAN FAMILY PLANORBIDAE. *By Frank Collins Baker. Collation, Revision, and Additions by Harley Jones Van Cleave. University of Illinois Press, Urbana.* $14.50. xxxvi + 530 pp. (including 141 plates); 1 portrait. 1945.

The value of a taxonomic monograph is determined largely by four factors, which may be summarized briefly as contribution, collation, convention, and availability. In this one, besides studies of the shells, with figures of most American forms and of examples of all the genera, the author has contributed careful descriptions of the detailed structure of the animals, with notes on nidification and development, all well illustrated in some 75 plates. These results of long and ardent years of dissection and study form a permanent record, and this book will remain an indispensable reference, long after new studies may have outmoded its systematic decisions. A chapter on general ecology includes a discussion of the planorbids as intermediate hosts for flukes.

The abundance of new data has been collated, with that from the past, into a revision of the family. After removal of the Bulinidae, the Planorbidae are divided into the Planorbinae with twelve genera, the Segmentininae with eleven, the Helisomatinae with seven, and the Planorbulinae with four genera. Definitions, lists of the (unindexed) "valid" forms, and notes on geographic and geologic distribution are given for each genus and subgenus. In addition, Lake Baikal's *Choanomphalus*, the mutative *Poecilospira*, *Indoplanorbis* (Bulinidae), and two genera "wrongly referred to Planorbidae" are discussed. The content of these groups, in terms of the affinities of the included individuals and minor entities, and their relationships, expressed in classification, give proof of the careful inductive reasoning and mature scientific judgment of the author.

In contrast to their content and relationships, the mere sizes of taxonomic groups are largely matters of convention, utility, and opinion. F. C. Baker here, as throughout his lifetime, frankly ranks himself with the "splitters," but even those colleagues, who may not agree with his opinions, must admit that his ability to see differences requires much more care and experience than does indiscriminate "lumping." A genus, two subgenera, nine species and seventeen varieties are described as new. In main, the legal conventions of nomenclature are respected, although *Menetus callioglyptus* (Vanatta, 1895) is included in *M. cooperi*, a "new name" in 1940.

My principal criticism is that the availability of this wealth of material is impaired by the inadequate indices, which exhibit a want of taxonomic experience, because of F. C. Baker's untimely death. But, despite this, Van Cleave, with his readable biography, careful bibliography, and excellent portrait, has constructed a fitting memorial to an eager searcher and a delightful friend.

HORACE B. BAKER

REPTILES AND AMPHIBIANS OF MINNESOTA.

By W. J. Breckenridge. The University of Minnesota Press, Minneapolis. $2.50. xiii + 202 pp. 1944.

It is a genuine pleasure to be able to aid in introducing a new state herpetology. This is one of the best reports that has appeared and emulates Conant's very excellent account of the Ohio reptiles (Conant, Roger. 1938. The reptiles of Ohio. *Amer. Midl. Nat.*, 20(1): 1–200). The book is designed for amateurs and for advanced students of herpetology; both should find in it much of interest.

A great deal of material is incorporated into the excellent Introduction, which includes sections on the following: history of Minnesota herpetology; debunking false stories; physical features of Minnesota; collecting methods; care of reptiles and amphibians (written by Milton Thompson); snake bite treatment; methods of preservation; classification; and keys with a description of their use. Following the Introduction is a Hypothetical List, which contains a discussion of those species which had previously been erroneously reported as occurring in Minnesota, and those which have not yet been found in the state but whose occurrence may reasonably be anticipated. The main body of the book is devoted to accounts of the separate species, which usually contain the following information: common name; scientific name; races occurring in Minnesota; description; range; habits and habitat; food; breeding; and one or two references for suggested reading. Each account is usually accompanied by an illustration and two small maps. The smaller map is of North America and shows in a general way the complete range of the form. On the larger county-outline map of Minnesota, in addition to certain vegetation features, the known range of the species is shown by counties. The book is concluded with a glossary, a bibliography, and an index.

The book is readable and the illustrations are acceptable. Especially to be commended are a couple of semi-diagrammatic scale drawings of frogs that should prove to be completely sufficient for accurate identification. Although the book is a very good one and will be useful to a great many persons, there is yet much to be criticized. Breckenridge has elected to treat the Minnesota herpetofauna on the species level. Each account is hence of a full species, and if more than one subspecies occurs in Minnesota this is noted under the heading, "races." Little attention, except in the case of *Thamnophis sirtalis*, is given such matters, and the result for the novice is bound to be one of confusion. For example, the form of the newt that occurs in Minnesota is *Triturus v. louisianensis* (this is correctly stated by Breckenridge) yet the legend under the figure accompanying the account is *Triturus viridescens*. The same situation is true for the genera *Pseudacris*, *Eumeces*, *Diadophis*, *Opheodrys*, and *Coluber*. Not only will this treatment trouble the amateur but it

will also disappoint herpetologists. One of the many functions of a state report should be to define the status of races within the state and map the areas of intergradation. Investigators planning generic revisions often look to these reports for such material, as they are rarely as familiar with the fauna as the author of such a manual is.

The keys (p. 27) contain some factual errors. In distinguishing between amphibians and reptiles it is stated that the former have a cartilaginous skeleton and the latter a bony skeleton whereas, of course, they both have a bony skeleton in common with all the air-breathing vertebrates. It is stated that lungs are well-developed in adult amphibians, although it is well known that in the Plethodontidae (an example of which occurs in Minnesota) the adults are lungless and depend upon cutaneous respiration for gas exchange. The statement (p. 73) that the color of the tree frog "is adjusted to blend remarkably well with its immediate surroundings" is open to question. Errors of this type are regrettable since they foster, in a few experimental zoologists, the attitude that some museum workers may be naturalists but are not biologists.

The book is good looking and typographical errors are very rare. It is to be hoped that a second edition will be warranted and that it will be sufficiently revised that the above remarks and other minor criticisms will no longer be pertinent.

ARNOLD B. GROBMAN

AMPHIBIANS AND REPTILES OF THE CHICAGO AREA. *By Clifford H. Pope. Chicago Natural History Museum, Chicago.* $1.75 (paper). 275 pp.; 12 plates. 1944.

All the publications of Clifford Pope are interesting reading and deserve the favorable reception they have been accorded. This book will prove to be no exception and will be highly regarded, along with his popular books on the snakes and turtles of the United States, his series of technical papers on the Appalachian salamanders, and his scholarly 600-page volume on the reptiles of China.

Here one finds a detailed yet readable treatment of the salamanders, frogs, toads, lizards, snakes, and turtles of the Chicago area, in which is included those parts of Michigan, Indiana, Illinois, and Wisconsin which border the southern tip of Lake Michigan and are within a seventy-mile radius of Chicago. After an all too short introduction, the amphibian and reptile groups are briefly considered, and the forms within each are discussed in adequate detail.

Carefully simplified keys for identification are provided. Technical terms are reduced to a minimum, and those that are used are satisfactorily defined, often with diagrams, where they first appear. Also aiding in identification are the many excellent drawings of the amphibians and lizards, the photographs of the snakes, and the beautifully executed colored plates of the turtles. From the front cover, coming out to meet the reader, is a Walt Disney Tiger Salamander. The same salamander may again be seen as it leaves the reader and enters the book through the back cover.

For each form included the following information is usually supplied: common name; scientific name; references to accompanying illustrations; a section on recognition which often includes the means of distinguishing it from species with which it has a strong superficial resemblance; a section describing differences between males and females; and sections on reproduction, growth and age, habits, food and feeding habits, enemies, economic importance, habitat, captivity, and occurrence. Under the last are given both the general distribution of the form and those counties of the Chicago area from which it is already known. In this regard another reviewer (Price, John W., *Sci. Monthly*, 60: 399–400, May 1945) has suggested more distribution maps and has decried the lack of specific locality records. In my estimation of readers' requirements and the efficient utilization of space, it seems that Pope's presentation would not be improved were the above suggestions incorporated. Concluding the account for each form are one or two annotated references.

The design of this book, "written for those already interested in natural history," should prove to be highly satisfactory both to the novice and the experienced herpetologist. Terminology is given careful attention, and with almost every statement there is an indication of its source. It is unnecessary to point out the ways in which this book will be found acceptable to the interested amateur, for it has apparently been successfully organized with his requirements foremost in mind. The more serious student of amphibians and reptiles will be pleased with the wealth of information that has been brought together. Discussions of intergradation, allocation of questionable specimens, and habitat preference are woven into the species accounts. The paragraphs on life histories and habits are very well done; those on courtship and reproduction are, perhaps, overdone. There is an extensive index.

The book is preeminently satisfactory and there is little to criticize. The paper and type have been well chosen and there is an absence of typographical errors. There is no bibliography, and all references in the text are limited to the names of investigators without mention of date or journal. The addition of a glossary and a section explaining the use of the keys would make the book more useful to the inexperienced naturalist.

ARNOLD B. GROBMAN

AMERICAN BIRD SONGS.

Recorded by the Albert R. Brand Bird Song Foundation, Laboratory of Ornithology, Cornell University. Comstock Publishing Company, Ithaca, N. Y. $6.00. 6 10′ records. 1944.

The recording of these 72 bird songs in the field was a feat of tremendous technical difficulty, and all students of birds will welcome them. They stand far above all previous efforts at recording, revealing the great technical advances that have been made in recent years. This is not to say, however, that the records are perfect and will satisfy the eager desires and sharpened criticism of all bird lovers. It is difficult to judge their quality objectively, since the reproducing instrument is a variable factor of considerable importance. However, after listening to them repeatedly on a good instrument, a few criticisms appear obvious. Variations in volume from song to song on the same record face are often great and make it difficult to keep the volume adjusted; technical monitoring could be improved. The repetition of the recorder's "plug" at the beginning of each disk soon becomes wearisome. The sparrows fare better in the recordings than most other groups. Whip-poor-will and chuck-will's-widow are fine. Yet some of the most familiar songs seem least recognizable—this is true of robin, catbird, mockingbird, and flicker. The tones seem over-harsh, short of their natural lovely liquidity; the songs are atypical, and a beginner could hardly learn to identify these birds from their recordings. Others are recorded admirably—the Canada geese calling, apparently in flight, are enough to make any hunter feel a keen pang of nostalgia. Others are rare or unusual and represent outstanding inclusions: the veery, the hermit thrush, the woodcock, the chachalaca. One entire record is devoted to birds of the West, with a few additional western birds, such as Gambel's quail and the California quail, on other records. The drumming of the ruffed grouse and the calls of dusky grouse and prairie chickens, and the gobble of the wild turkey contribute exciting moments. In spite of the fact that perfection in bird recording is still to be achieved, this album will richly reward all ornithologists and bird lovers.

BENTLEY GLASS

BIRDS OF THE SOUTHWEST PACIFIC. *A Field Guide to the Birds of the Area between Samoa, New Caledonia, and Micronesia.*

By Ernst Mayr. The Macmillan Company, New York. $3.75. xx + 316 pp.; 3 plates. 1945.

As a result of the war in the Pacific Area, museums have been flooded with requests for information on the flora and fauna of the far-flung and exotic islands by individual service men. Queries on bird life have been especially frequent, but most of the information lies scattered in technical journals and special reports not accessible to the fighting man. In response to the demand, Mayr has written this handbook of birds to be found in the following islands: Samoa, Fiji, Tonga, New Caledonia, Loyalty Islands, New Hebrides, Banks Islands, Santa Cruz Islands, Solomon Islands, Marshall Islands, Carolines, Marianas, Palau, Yap, Guam, Gilbert and Ellice Islands, and the Phoenix Islands.

In setting up a plan of presentation, the author has had quite a different problem confronting him than is met with in similar types of faunal handbooks. The area encompassed is tremendous, consisting of thousands of diversified and partially isolated islands. Furthermore, although the avifauna in a taxonomic sense may be well known, very little information is at hand on field identification, habits, and habitats. Lastly, the birds in many instances are vastly different from those that a North American ornithologist might be acquainted with. The author has resolved these difficulties very cleverly. The book is divided into two parts, the first of which describes the seabirds of the Southwest Pacific, the shorebirds, and the land and fresh-water birds. Since the first two groups consist of birds rather cosmopolitan in habit and distribution, they are described only once and their distribution recorded. But in the third group, in order to acquaint the student with the families peculiar to the Australian region, there is given a short description of each of the families found in the Southwest Pacific. Included in this section are species of widespread distribution (occurring in three or more of the seven geographical subdivisions). Part two is the geographical section listing the birds of the seven subdivisions.

Field keys are given when it is believed they will be of value in identification; color plates and black and white drawings include characteristic species of that region. The entire volume is a creditable piece of workmanship that will be of value not only to persons in the Southwest Pacific but also to others interested in the avifauna of these islands, either professionally or otherwise. The value of some of the features used in the keys as diagnostic recognition marks may be questioned; perhaps a few more illustrations might have been more helpful. But as the author continually points out, field information is scanty, and he encourages any information from those who use the book. The introduction contains hints to observers on the type of data that is badly needed.

HENRI C. SEIBERT

WOODCOCK.

By John Alden Knight. Color plates from paintings by Edgar Burke. Alfred A. Knopf, New York. $4.00. x + 161 + ii pp.; 16 plates. 1944.

The twilight flights of the male woodcock rank fairly high among the striking courtship displays of birds, and are relatively well known; but even of those who have watched this performance, few know much about the rest of the woodcock's exceedingly private life.

This book, though it makes no claim to be a scientific study, does contribute considerably to our knowledge of the bird's habits. It discusses such surveys and preliminary banding experiments as had been carried out by the U. S. Fish and Wild Life Service and State Game commissions before the war, and states clearly what is known and what remains to be discovered about the seasonal cycle and migratory movements of the woodcock. Years of first-hand observation have enabled the author to sift facts from sportsmen's tales, and his conclusions about the type of cover selected by the woodcock are particularly interesting.

As his observations were made primarily in the capacity of a hunter, the author devotes a considerable part of the book to reminiscences of woodcock shooting and discussion of equipment and technique. There are some good illustrations of the bird and its haunts.

SUMMARY OF THE COLLECTIONS OF AMPHIBIANS MADE IN MEXICO UNDER THE WALTER RATHBONE BACON TRAVELING SCHOLARSHIP. *Proceedings of the United States National Museum, Vol. 95, No. 3185.*

By Edward H. Taylor and Hobart M. Smith. Smithsonian Institution, United States National Museum, Washington. Paper. Pp. 521–613, plates 18–32. 1945.

RESULTS OF FEEDING ETHER EXTRACTS OF MALE SUPPLEMENTARY REPRODUCTIVES TO GROUPS OF NYMPHAL TERMITES. *University of California Publications in Zoology, Volume 49, No. 9.*

By E. A. Keene and S. F. Light. University of California Press, Berkeley and Los Angeles. 25 cents (paper). Pp. 283–290. 1944.

ANIMAL EMBRYOLOGY

THE EMBRYOLOGY OF BEHAVIOR: *The Beginnings of the Human Mind.*

By Arnold Gesell, in collaboration with Catherine S. Amatruda. Harper and Brothers, New York and London. $5.00. xix + 289 pp. 1945.

"He who sees things from their beginnings," quotes Gesell in the first paragraph of his *Embryology of Behavior*, "will have the most advantageous view of them." Accordingly, in compliance with the admonition of Aristotle, the author has made a brave attempt to trace the human mind to its ultimate embryonic origins.

Gesell has borrowed liberally, from the appropriate authorities, pertinent data concerning early human development. He has devoted nine of his fifteen chapters, over half his textual pages, in fact, to epitomizing and interpreting these for his readers, concentrating his attention on the development of structure and function in the motor and coordinatory apparatus, and particularly on the mechanisms regulating the breathing, tonic, and integrative processes. He has then presented, in three chapters, his original observations on the development of behavior in premature as compared with full-term infants. In his final three chapters, he has concluded by attempting to derive from the material presented a unifying concept synthesizing biological and psychical phenomena into a single scheme of things entire. At the end of the text follow forty-four plates representing "a photographic delineation of behavior patterns and growth sequences" in premature and normal infants, and two brief appendices elucidating a few of the more technical aspects of the author's original investigations.

In his treatment of data borrowed from biology, embryology, and neurology, the author is on the whole accurate and sound. In his adoption of interpretations and concepts, rather than facts, from cognate fields, he is open to reproach only for his own lack of critical skepticism. The concepts of metabolic gradients, of electrodynamic fields, of electrotonic integration, are convenient ways of describing in fresh terms physiological phenomena already familiar in other guises. Whoever accepts them unreasoningly as more than descriptive in scope runs the risk of confounding cause with effect; the critical scientist cannot accept them as ultimate explanation. Gesell, however, in appropriating these concepts, is no more at fault than the great majority of biologists and physiologists, who are themselves unfortunately still all too eager to grasp after such pale phantoms of truth.

The present reviewer is perhaps not wholly competent to evaluate the worth of the author's original contributions. The corpus of data, as presented in this volume, however, seems a little thin against the rich background of borrowed materials, and in some ways as insubstantial in structure as those happy pavilions in Flushing which neighbored Dr. Couney's excellent hospital where the basic facts were collected. The chapter describing the development of the "diurnal cycle of sleep and wakeness" is decisively analytical and intensely interesting. The descriptions of the development of other behavior patterns may be adequate in their fashion, but they lack the scientific precision and objectivity of observation and of expression that distinguish, for instance, the developmental studies by Davenport Hooker.

This deficiency may be partially accounted for by the author's predilection for playing with words, a practice in which he indulges too freely. To this reviewer, for instance, the word "growthsome," the

terms "semi-demi drowsing" and "micro-munching," the phrase "genes, arranged warp-woof-wise in chromosomes," although admittedly expressive in a lyrical sort of way, are still objectionable. Gesell's style is good, but it is not that of his heroes Coleridge and Melville—nor even that of Sherrington—and might better not have succumbed even spottily to their literary influence.

The final synthesizing chapters, especially, are weakened by the author's experiments with language. He has used in his text the phrase "a kind of cauliflower overgrowth," and the simile might be extended to cover the manner in which he shrouds relatively simple ideas with a plethora of words. Experimental embryologists who have transplanted organizers, for instance, may resent being told by Gesell that their experiment "demonstrates the profoundity [sic] and inveterateness of the creative essence of growth." Nor does the author necessarily enrich the existing body of knowledge by conceiving of the development of behavior as governed by the seven principles of "individuating fore-reference," "developmental direction," "spiral reincorporation," "reciprocal interweaving," "functional asymmetry," "self-regulatory fluctuation," and "optimal tendency." Surely these principles could be more simply stated, to the greater benefit of the scientist who might choose to employ them in his own thinking.

But if Gesell's scientific effort seems a trifle shallow, and his literary phraseology over-elaborate, on the philosophical level he stands above reproach. He has followed the current fashion of thinking in terms of levels of integration, and has developed a concept of a "hierarchical continuum" in which behavioral and psychical phenomena are directly, through their development, related to physical phenomena of a lower order. By so incorporating the properties of mind and matter into a single continuous system, he localizes the mind into a position in nature in which it can be analyzed by the established methods of science. The Trylon and Perisphere have vanished, in whose shadow the Doctor's subjects squinched and munched; but by thus emphasizing on an embryological basis the fact that the human mind can be rendered scientifically comprehensible, Gesell helps to hold open the way to a more fruitful future World of Tomorrow.

JANE OPPENHEIMER

EXPECTANTLY YOURS. *A Book for Expectant Mothers and Prospective Fathers.*

By Mario A. Castallo and Audrey Walz. The Macmillan Company, New York. $1.75. xiii + 110 pp. 1943.

Long before the printed word, books were written to give insight and advice to the layman in regard to pregnancy and parturition. The great first century A.D. Greek text of Soranus on obstetrics and gynecology was apparently written for physician, midwife, and literate layman alike. When medicine was so little a specialized science, the educated layman had little or no difficulty in translating the verbiage or the thought processes of medicine into his own intelligence. This is not true of modern medical science, for it is explained in terms and disciplines that are only intelligible to the fully initiated, and often to them only with the aid of a trade dictionary. This is not stated as a criticism but as a fact. The obvious result is the recent publication of a whole library of popular books on various matters medical. The largest shelf of this library is given over to insight and advice books on the stork and his exciting avocation. Certainly this is natural enough, for no major medical phenomenon affects so many people so often.

Castallo, Assistant Professor of Obstetrics at Jefferson Medical College, and Gynecologist to St. Agnes and to St. Mary's Hospital, is the latest author to try his skill at popularizing medicine. The resultant book, *Expectantly Yours,* is a highly creditable performance both in its contents and form. It is illustrated with excellent pen and ink sketches by Helen G. Schad.

In the opening chapter the author allays the fears of his readers by stressing the current low mortality figures, which would even be "a third lower" if immoral abortions were not included. He further points out to his readers that those who do not read such books, "the underprivileged one-third of a nation who eat too little and work too long and hard," contribute more than their third to the death toll. Castallo could have contrasted the mortality figures of the white and colored groups to make this point even sharper, but he did not.

In the seven brief pages of Chapter Two, "The Facts of Life," the author reveals the bare essentials of conception, sex-ratio, and maternal impression. Next follows a discussion of the initial visit to the obstetrician, and the training rules he lays down. In "Mind over Matter" the author tries to argue the pregnant woman out of hypersensitivity to her anterior bulge. In his eyes she simply exchanges her non-pregnant glamour for a spiritual beauty; she metamorphoses from a Varga Esquire girl to the central figure of Botticelli's "Spring." In too "polyannish" a vein he glosses over the chances for abnormal offspring. The book suffers from a lack of frankness about the unpleasant, which I think would intellectually insult me were I the lay reader. The fact that I am not the ordinary pregnant reader and can never be, makes the latter statement of uncertain value.

Thirty pages are devoted to labor and delivery. Such practical details as to what to take to the hospital for mother and baby are included, as well as a discussion of spontaneous birth, forceps delivery, and Cesarean

section. The matter of pain relief in labor is discussed in relative detail. Fifteen pages are devoted to the puerperium (the "come-back period"). Exercises to strengthen the abdominal musces are well depicted in line drawings with appropriate descriptions beneath. The emotional redjustment to include another loved one beside the husband, and its sexual sequelae are treated very sympathetically. The concluding chapter is concerned with the technique of infant care: feeding, diapering, and bathing.

Castallo's book is authoritative and accurate. It is written in an informal, almost breezy style, always addressing the reader with the personal pronoun "you." The facts are presented in a naked, pleasant, and concise form, with few historical, literary, or statistical trimmings. I think it is ideal for the unsophisticated reader, but I doubt whether a college major in biology would find it intellectually satisfying in the quest for knowledge of her pregnant self.

ALAN F. GUTTMACHER

ANIMAL MORPHOLOGY

ANATOMY *As a Basis for Medical and Dental Practice. By Donald Mainland. Paul B. Hoeber Inc., New York and London.* $7.50. xvii + 863 pp. 1945.

This new textbook of human anatomy is both unusual and interesting, neither of which adjectives can be applied to many texts of gross anatomy. Three underlying tenets appear in the writing of this book: first, the definition and interpretation of anatomical structures in view of their function; second, the organization of such data into topographical and physiological systems; and third, the presentation of facts in a manner to encourage the student to *think* Anatomy rather than to memorize it.

There are two main parts of the book. The first and shorter part (152 pages) is a general, correlative discussion of the systems of the body. Mainland has taken pains to point out repeatedly that gross anatomy is not a dogmatic or a static science. He has done this by placing considerable emphasis on the omnipresence of variation, both human and comparative, and on the relation of structure and function. The author's method of argument is well illustrated by his discussion (pp. 80–84) of the factors, influencing the inner structure of bones, especially the formation of trabeculae as formulated by Wolff. The criticism of the Wolff doctrine brings sharply to the reader's mind that the factors regulating bone structure are diverse, variable, and not by any means fully understood.

The second section of the text (586 pages) is topographic in organization, but the same insistent questioning tends away from a cut-and-dried presentation. The text throughout is sprinkled with references to the literature, and the length of the bibliography (33 pages) indicates a broad coverage of the field. This contrasts with the last edition of one of the standard textbooks of anatomy, which contains only 149 references to the literature, which, if gathered together, might amount to 5 pages, and among which the most recent paper appended to the chapter on the skeletal system was written in 1923!

In attempting to evaluate this new text, it must be borne in mind that it is to be used in conjunction with an atlas, and probably with a standard text as a reference. The book is a radical departure from the usual approach, and consequently will not be welcomed by those who abhor any change in method. It seems that in the attempt to simplify and condense, the author has catalogued the material in a manner which makes for choppy reading. The dynamic, *biological* approach, however, will not only make the subject interesting to the student, but will enable him to evaluate from the anatomical side the reversible equation of structure and function. In such an understanding lies the future of anatomy, both for the surgeon and practitioner, and for the research worker.

JAMES M. SPRAGUE

STRUCTURE AND FUNCTION AS SEEN IN THE FOOT. *By Frederic Wood Jones. William Wood, Williams and Wilkins Company, Baltimore.* $7.50. iv + 329 pp. 1944.

This volume does not possess the stimulating originality of the previous books by Frederic Wood Jones (*Arboreal Man*, 1916; *The Principles of Anatomy as Seen in the Hand*, 1920 and 1942; *Man's Place among the Mammals*, 1929), but it is a useful, detailed compilation of facts, describing the macroscopic anatomy of the foot of man, with special reference to function. For an anatomical treatise it is unusually lively and readable, partly because it includes many topics not to be found in standard textbooks of human anatomy. Thus, there are chapters dealing with the principles of terminology, the human foot in phylogeny and in ontogeny, the digital formula, the metatarsal formula, and external characters, such as flexure lines, cleavage lines, papillary ridges, and hair, but not a word on nails. The largest part of the book is devoted to the discussion of bones and muscles (195 pages), nerves and blood vessels receiving less attention (58 pages). The treatment of these subjects gains a great deal by frequent references to individual variations, age changes, racial differences, etc., which are all too often neglected in our textbooks.

Throughout this work the author attempts to support his pet hypothesis that "in the anatomy of the human foot there is abundant evidence that it is derived from a primitive mammalian foot, retaining its primitiveness probably because of the arboreal habit. Such a foot we have every reason to believe belonged to the primitive members of the Primate stem—the stem from which Man and the other existing members of the

Primates branched apart and went their separate way at a very early stage in the Primate story." In pleading his case the author conveniently ignores many observations, recorded in recent literature, which constitute good evidence against his untenable claim.

The "References for Further Reading," appended to each chapter, include many titles of historical interest, but they omit a considerable number of significant contributions of the last two decades. The one hundred and fifty somewhat crude and diagrammatic illustrations could have been greatly improved with little trouble by a competent illustrator.

In spite of its shortcomings (and its high price) the book as a whole forms a welcome and useful addition to the literature of human anatomy.

A. H. SCHULTZ

THE PRECENTRAL MOTOR CORTEX. *Illinois Monographs in the Medical Sciences, Volume IV, Nos. 1–4. Edited by Paul C. Bucy. The University of Illinois Press, Urbana.* $5.50 (cloth); $4.50 (paper). xiv + 605 pp. 1944.

Exactly seventy-five years have elapsed since Fritsch and Hitzig first demonstrated the existence of an excitable motor area by directly stimulating the cerebral cortex of mammals. Since that time, largely by virtue of the techniques of stimulation and ablation, and more recently by action potentials, a great mass of data has been collected and synthesized. This volume, under the editorship of Paul Bucy, presents the current status of our knowledge of the cerebral motor cortex. It serves not only to bring together the scattered observations in convenient form, but also to indicate the gaps in existing knowledge and the more likely directions of future research. The seventeen original chapters have been written by fourteen authors, all of whom have made major contributions to one or more aspects of the subject. John F. Fulton has written a "Foreword."

Bucy, in his "Introduction," defines the precentral motor cortex as "the principal efferent or effector cerebral cortical mechanism by which the brain expresses its activity through the skeletal musculature. The portion of the precentral region principally so engaged is the cytoarchitectonic areas 4, 6, and 44."

Gerhardt von Bonin discusses the "Architecture of the Precentral Motor Cortex and Some Adjacent Areas." This includes many new, hitherto unpublished observations. Noteworthy among his conclusions are recognition of a layer IV in the precentral agranular cortex and the identification of an area 44 (Broca's area) in all of the non-human primates which he has studied (*Galago, Cebus, Macaca,* chimpanzee). It formerly had been thought, on the basis of Brodmann's work, that this area was peculiar to man. Brodmann regarded this region as belonging to area 6 in other primates. If von Bonin's interpretation is substantiated, then a "speech area" is common to primates in general and so distinguishes them from other mammals.

James L. O'Leary deals with "The Role of Architectonics in Deciphering the Electrical Activity of the Cortex." "Afferent Connections" are treated by A. Earl Walker; "Efferent Connections" by Paul M. Levin; "The Pyramidal Tract" by Sarah S. Tower; "Cortico-cortical Connections" by Warren S. McCulloch; "Somatic Functions" and "Autonomic Functions" by Margaret A. Kennard; "Relationship to the Cerebellum" by Percival Bailey; "The Frontal Eye Fields" by Wilbur K. Smith—the latter, although not included in the term precentral motor cortex, since they occupy area 8, are considered here because they are concerned with the innervation of striated muscle and also function as a suppressor area having many of the characteristics of area 4s. "Electrical Excitability in Man" is discussed by Theodore C. Erickson; "Effects of Extirpation in Man" and "Relation to Abnormal Involuntary Movements" by Bucy; "Clinical Symptomatology" by Charles D. Aring; "Pathology" by Charles Davison; and "Significance of the Precentral Motor Cortex," an appraisal of the results of stimulation and ablation, by Marion Hines. Respecting the question whether the representation in the motor cortex is of individual muscles or of movements, Hines points out that the two views are not incompatible. Chapter VII is a translation by von Bonin and McCulloch of Bubnoff and Heidenhain's old but significant paper, "Ueber Erregungs- und Hemmungsvorgänge innerhalb der motorischen Hirncentren" (Arch. ges. Physiol., 1881, 26: 137–200). There is a bibliography of 797 titles.

One is impressed, after going through this splendid volume, by how little of our existing knowledge of the precentral motor cortex has been secured from direct observations of man. This, of course, might be expected in view of the many difficulties involved. The chief source of recent knowledge has been the rhesus monkey, and, in considerably less degree, the chimpanzee. One of the urgent problems is how to secure more and better data for man. All catarrhine primates no doubt agree in many fundamental details, but it scarcely need be stressed that striking species differences are to be expected.

W. L. STRAUS, JR.

ATLAS OF THE BLOOD IN CHILDREN. *By Kenneth D. Blackfan, Louis K. Diamond, and C. Merrill Leister (illustrations). The Commonwealth Fund, New York.* $12.00. xiv + 320 pp., including 70 colored plates. 1944.

In his foreword, George N. Minot points out the great need for pictures of the blood of children, for, as he notes, the blood of the child shows a much greater

variability than does that of the adult. "Lability of children's blood is the rule. . .and the younger the individual the greater the instability." This tends to make diagnosis or evaluation of the blood picture in the child more difficult than in the adult. Furthermore, the same stimuli may produce quite different blood pictures at the two stages of life.

This handsome volume, subsidized by the Commonwealth Fund, more than satisfies this need. It is based upon observations on more than 5,000 infants and children (many followed for several years) recorded by the late Dr. Blackfan and by Diamond over a period of many years. The text (144 pages) comprises useful, brief accounts of the various blood diseases—etiology, symptoms, laboratory picture, course, prognosis, and treatment. Case histories are included. It is divided into 5 chapters: (I) Blood Cells—a good introductory account of normal blood cells, their life history, and hemopoiesis; (II) The Erythrocytes in Anemia (the normocytic normochromic anemias, the macrocytic hyperchromic anemias, the microcytic hyperchromic anemias, the anemias associated with jaundice); (III) The Leukocytes in Disease (leukocytosis and leukopenia, neutrophilic leukocytosis, neutropenia, eosinophilic leukocytosis or eosinophilia, lymphocytosis and lymphopenia, monocytosis); (IV) Leukemia; (V) The Platelets (thrombocytopenia, thrombocytosis and thrombocythemia). A bibliography of 14 pages is appended.

The chief glory of the volume are the superb drawings of Wright's stain preparations of normal and diseased blood by Leister, who represents the rare combination of pediatrician and artist. These are beautifully reproduced as 70 lithographic plates in eight colors, with excellent accompanying keys and legends.

This atlas will recommend itself not only to pediatricians, but to others—such as general medical men, hematologists, pathologists, and anatomists—as well. For there really is nothing like it, and its usefulness extends well beyond the relatively narrow implications of its title.

As a sort of a castle in Spain, may one be permitted to wonder if the utility of such a work would not be enhanced for one of its avowed purposes—to assist medical students and physicians in the identification of cells in blood diseases—if illustrations of mediocre blood films had also been included? This may sound like heresy, and malapertness toward Leister and his fine illustrations. But these indeed have been taken from exceptionally fine films. Anyone who has tried to prepare blood with Wright's or similar stains knows that it is not the lot of other than the expert or near expert to attain such excellence; hence it is the ordinary, mediocre preparation that most frequently will require interpretation.

W. L. STRAUS, JR.

ANIMAL PHYSIOLOGY

ANNUAL REVIEW OF PHYSIOLOGY. *Volume VII. 1945.*

Edited by James Murray Luck and Victor E. Hall. The American Physiological Society and Annual Reviews, Inc., Stanford University P. O. $5.00. viii + 774 pp. 1945.

This annually published volume of reviews has become indispensable to the physiologist and has been welcomed by the personnel of other branches of biological science. The volume published in 1945 contains twenty-six articles, which cover the literature published in these fields during the last three to five years. Subjects reviewed are permeability, the physical properties of protoplasm, the physiological effects of radiant energy, physiological aspects of genetics, developmental physiology, water metabolism, physiological effects of heat and cold, energy metabolism, the peripheral circulation, respiration, muscle, digestive system, kidney, blood, the lymphatic system, heart, electrical activity of the brain, conduction and synaptic transmission in the nervous system, the somatic functions of the nervous system, the special senses, metabolic functions of the endocrine system, hormones in reproduction, exercise, physiological psychology, applied physiology and pharmacology. These reviews vary enormously in scope and type. A few of the reviewers because of the more specific and limited nature of their topics, were able to give an analysis of the present state of knowledge of certain functions. In the majority of cases, however, the fields to be covered were so large that the reviewers could do little more than list and classify recent contributions. The chief value of this volume resides in the fact that it will be of great assistance to those who wish to keep informed concerning a massive and varied literature. The contributors, for the most part, have justified the assumption that in their summaries they would not fail to describe all pertinent articles of recent date.

CHANDLER MCC. BROOKS

PHYSIOLOGY IN HEALTH AND DISEASE. *Fourth Edition, thoroughly revised.*

By Carl J. Wiggers. Lea and Febiger, Philadelphia. $10.00. 1174 pp. 1944.

This is the fourth edition of one of the advanced modern textbooks of physiology. The author has attained a high degree of success in his effort to bring various discussions into accord with the physiological thinking of the day. On reading this book one is constantly impressed by the author's ability to summarize and present the mass of information now available in the numerous and diverse fields which students of physiology must consider. The topics covered in the nine sections of this book are: (1) muscular contraction and

associated phenomena; (2) the physiology of the peripheral and central nervous systems; (3) the blood; (4) respiration; (5) the heart and circulation; (6) physiology of the alimentary tract; (7) metabolism and nutrition; (8) excretion and body fluids; and (9) the endocrine organs. Decisions reached in discussions of controversial subjects are frequently different from those described in other textbooks of like scope, but references are given which permit the reader to find the available evidence and to form his own opinion if he so desires. Each subdivision of the major sections of the book contains an explanation of some physiological mechanism, and this is generally supplemented by a brief summary of the abnormalities of function consequent to various pathological processes. The sections which deal with the mechanism of the function of the cardiovascular system are very well conceived and clearly written. Those textbooks in which the various sections have been contributed by men who have specialized in those particular fields convey a somewhat higher degree of authority and attain a greater completeness of presentation, but they generally sacrifice uniformity of performance and clarity, qualities which this textbook possesses.

CHANDLER McC. BROOKS

LABORATORY MANUAL FOR ELEMENTARY PHYSIOLOGY. *Fourth Edition.*

By Lalia V. Walling and Kenneth Siler. The C. V. Mosby Company, St. Louis. $1.50 (paper). 187 pp. 1945.

This laboratory manual contains 116 exercises which may be performed with simple equipment by students who have had little previous training in the biological sciences. The thirteen major topics treated are: the physiology of muscle and nerve, the central nervous system, sense organs, the circulation, blood, respiration, and the digestive system. Considerations of chemical tests for foods, metabolism, heat regulation, and excretion are likewise included. The procedures outlined are much too elementary for advanced students but may be suitable for college courses in physiology. The number of exercises seems to be rather excessive, and one wonders whether it would be possible or, for that matter, of value to the students to perform all of them. If properly supplemented with correlated lectures, such a laboratory course as is outlined in this manual would aid in giving students a good though elementary knowledge of certain body functions.

CHANDLER McC. BROOKS

LABORATORY NOTES IN PHYSIOLOY. *Second Edition. Compiled by William R. Amberson, Dietrich C. Smith, Robert H. Oster, Robert S. Anderson, Joye J. Jennings, and Mary E. Chinn. The Williams & Wilkins Company, Baltimore.* $2.25. iv + 91 pp. 1944.

This brief laboratory manual contains descriptions of twenty-nine experiments, which have been classified under five headings. The experiments of section I deal with muscle and nerve, those of section II with the central nervous system and the special senses. Section III contains a series of mammalian experiments that include studies of the cardiovascular system, respiration, and the secretion of saliva, bile, and pancreatic juice. Sections IV and V deal with respiration, metabolism, circulation, and water balance in man. All of these experiments are of value, they involve procedures which are not too complicated for successful performance by the average medical student, and the results which are sure to be obtained are highly instructive. No more need be said than that this is a well conceived and well written guide which could be employed in any laboratory engaged in the teaching of physiology to advanced students.

CHANDLER McC. BROOKS

YOUR DIGESTIVE SYSTEM.

By Louis Winfield Kohn. The New Home Library, The Blakiston Company, Philadelphia. 69 cents. xiii + 274 pp. 1944.

The primary purpose of the author of this book is to acquaint the common reader with the various factors involved in the production of digestive disorders, and to help him note the many symptoms which indicate the need for medical attention. Consequently, a brief introductory summary of the anatomy and function of the digestive tract is followed by a series of chapters which describe the diseases of this system. Information is given concerning the diagnosis, nature, and consequences of the diseases which commonly occur in various portions of the gastro-intestinal tract. Descriptions are sufficiently simple and clear to permit the common reader to greatly increase his knowledge of this important system of organs and the symptoms of their pathology. It is hoped, however, that reliance on books of this type will cause no one to question the advisability of seeking periodic examinations by a qualified physician. The absurdly diagrammatic illustrations detract from the respectability of this publication. For example, no value can be attached to illustrations which locate the liver, gall bladder, pancreas and stomach within the thorax (pp. 28, 110, 136, 201, etc.).

CHANDLER McC. BROOKS

YOUR RESPIRATORY SYSTEM.

By Herman Pomeranz. The New Home Library, The Blakiston Company, Philadelphia. 69 cents. xvii + 298 pp. 1944.

This book is intended to contain basic information of a scientific nature, so presented as to be readily understood by the general reader. The author's stated aim was to include in one volume a non-technical description of the respiratory system, its structure, functions, diseases, treatment, and hygiene. These aims have been partially attained; the book does contain a thorough and simple description of the anatomy and pathology of the respiratory organs. However, comparatively little is said concerning the control and function of these structures. A large proportion of the volume is devoted to a description of diseases of the lung, early symptoms, diagnostic methods, and treatment. Since the emphasis of the entire work is on pathology, the chief value of the book probably lies in the fact that it does contain easily understood descriptions of the consequences of abnormalities of various structures of the system and descriptions of the signs of developing pathology. The reader consequently might be more intelligent in his decision as to when he should seek the aid of a physician. In addition, the book has some value as a reference work, but one may doubt whether the general reader will be sufficiently interested in the subject matter to voluntarily read it completely. Facts are accurately stated for the most part, but there are a few errors, such as the statement that "an intrapulmonic air pressure of over 60 millimeters of mercury will cause a separation of the pleura from the lung and result in an air bubble." The intrapulmonic pressure normally varies between a few millimeters below and above 760 millimeters of mercury and when one forcibly expires, pressures may rise to 860 millimeters (100 mm. above atmospheric) without damage to the lung. One also tends to question the statement that a given area of nasal mucous membrane gives off seven times as much carbon dioxide as a similar area of alveolar wall, but the book gives no clue as to the sources of the author's information. In summary, it can be said that the book is acceptable, and that the reviewer enjoyed reading it and learned numerous things during this study.

CHANDLER MCC. BROOKS

MANOMETRIC TECHNIQUES AND RELATED METHODS FOR THE STUDY OF TISSUE METABOLISM.

By W. W. Umbreit, R. H. Burris, and J. F. Stauffer. Burgess Publishing Company, Minneapolis. $3.50 (paper). iii + 198 pp. 1945.

This manual is a compilation of techniques which are commonly used in the study of tissue metabolism. As described, they are adequate for studies undertaken by graduate students working under the direction of a mature investigator. Experienced workers will welcome this manual as a handbook to aid them in broadening and improving certain of their technical procedures.

The authors have attained their declared aim in describing methods which have been well tested and are commonly employed, which require a minimum of equipment and only that equipment available in biological laboratories and which, though not describing manometric measurements, are essential to the study of tissue metabolism. The topics dealt with in the sixteen chapters of the book will give one a clear idea of the great value of this manual to those who are interested in obtaining knowledge of the metabolic processes of individual tissues. The chapter titles are: (1) the Warburg constant volume respirometer; (2) the "direct method" for carbon dioxide; (3) carbon dioxide and bicarbonate; (4) the "indirect" method of Warburg; (5) useful techniques for use with manometric methods; (6) determination and use of flask constants; (7) the differential manometer with special reference to its use in studies of photosynthesis; (8) methods of tissue preparation; (9) the homogenate technique; (10) chemical methods; (11) special methods employing manometric techniques; (12) "Thunberg" techniques for estimation of dehydrogenase activity; (13) related electrometric techniques; (14) manometric estimation of metabolites and enzyme systems; (15) methods for the analysis of phosphorylated intermediates; (16) preparation of physiologically important intermediates and metabolites. This book is recommended to all those interested in this aspect of physiological investigation.

CHANDLER MCC. BROOKS

DIE HORMONE: *Ihre Physiologie und Pharmakologie. Erster Band: Keimdrüsen, Hypophyse, Nebennieren. Zweiter Band: Schilddrüse, Nebenschilddrüsen, Inselzellender Bauchspeicheldrüse, Thymus, Epiphyse.*

By Paul Trendelenburg. Volume 2 edited by Otto Krayer. (Julius Springer, Berlin); J. W. Edwards, Ann Arbor, Michigan. $9.00 (vol. 1); $11.50 (Vol. 2). xi + 351 pp. (vol. 1); x + 502 pp. (vol. 2). (1929; 1934); 1944.

The first volume of Trendelenburg's *Die Hormone* originally appeared in 1929 with chapters on the gonads, the pituitary, and the adrenal glands; the second volume, dealing with the thyroid and parathyroid, pancreas, thymus, and pineal glands appeared in 1933, postdating the death of the author in 1931. Both volumes have now been reprinted by the Alien Property Custodian. Each chapter in these volumes is monographic in scope, and both the older literature and that to date of publication are covered with considerable thoroughness. It may not be amiss, then, to consider these volumes briefly in the light of developments that have occurred subsequently to their first appearance.

Volume I in particular demarcates a break between old and new trends. It was published in the same year

that the isolation of the female sex hormone by Doisy and by Butenandt opened up the field of the steroid hormones to the pure chemist, and, for the biologist, replaced crude organ extracts by known chemical entities. Meanwhile, emphasis shifted from adrenal medulla to adrenal cortex, and from the posterior pituitary to the anterior lobe. The extension of pituitary control to metabolic processes postdates Trendelenburg's chapter on the pituitary gland; however, the early work of Houssay that initiated this development is dealt with in some detail in the chapter on the pancreas. Those portions of Volume I, considerable in scope, dealing with the pharmacological properties of posterior pituitary extracts, and of extracts of the adrenal medulla, and adrenalin, together with large portions of Volume II, seem less dated by the passage of a decade. The thymus and the pineal body are with us still, obscure and not a little obfuscated. The hormone of the pancreas, produced in immense amounts annually, yields its secrets reluctantly, and advances in pancreas physiology have largely concerned its interrelationships with the pituitary and adrenal glands. The parathyroid hormone has proven refractory. In the thyroid field, the main advances to be recorded are the application of radioactive tracers to the kinetics of iodine uptake, and of histochemical techniques and microdissection to the study of colloid deposition and removal. Recently the effects of the pure chemical substances, thiouracil and alloxan, on thyroid and pancreas hormone secretion, respectively, have been exploited as techniques for the study of these hormonal mechanisms. The decade has witnessed some real progress in the isolation and definition of protein hormones of the anterior pituitary body, and of the pituitary-like substances of blood and urine.

These volumes of Trendelenburg contain much material of permanent value to the endocrinologist and pharmacologist, besides forming useful markers along the line of progress. They can be recommended as systematic introductions to the subjects considered, and for their coverage of literature that does not neglect the historical contributions in the various fields.

H. R. CATCHPOLE

THE MALE HORMONE.

By Paul de Kruif. Harcourt, Brace and Company, New York. $2.50. 243 pp. 1945.

This is the tenth of Paul de Kruif's books in the field of popular medical science. In it he succeeds again in dramatizing apparently dull facts and in presenting them understandingly to the general reader. Unfortunately, the book is less successful than the others, notably *The Microbe Hunters*. De Kruif has here, as often before, overdramatized because of his own self-admitted enthusiasm, doubtless accentuated in this instance by his personal experience with the beneficial effects of the drug he is discussing. Over-enthusiasm has often been dangerous in endocrinology because of the great gap between the more extravagant claims for the hormones and their actual benefits in therapy. Generally, however, De Kruif has managed to temper his enthusiasm with warnings as to some of the limitations of hormone treatment.

Can it be that testosterone is responsible for the somewhat lusty and ribald flavor of this book, and its vocabulary involving slang from the street and the laboratory? The combination has produced an unfortunate tone of vulgarity that vitiates De Kruif's purpose of presenting the case for testosterone without reticence and without prudery.

The author has used his book as a platform on which to air some of his prejudices. Many pages are devoted to a eulogy of Dr. Herman Bundesen of Chicago, whose chief claim to mention here is that it was he who first persuaded De Kruif to take testosterone. Throughout the book much space is devoted to presenting the investigators as the shining knights of medicine and the practitioners, particularly the specialists, as the black dragons who have killed progress and are more interested in death than in life. This sort of thing succeeds more in irritating than in being constructive. Sir William Osler, for instance, whose teachings have inspired so many to contend against death with skill and courage, is charged with a generally defeatist attitude toward serious disease and with being responsible for stamping "a mood of resignation against death upon modern medicine." Such prejudices serve to obscure the soundness of De Kruif's basic criticisms.

The Male Hormone, however, remains an exciting account of the search for testosterone and its development into a valuable therapeutic agent. Once more De Kruif has demonstrated his ability to vivify for the general public the selfless and often undramatic devotion of the investigator.

GEORGE W. CORNER, JR.

ELECTROENCEPHALOGRAMS OF NORMAL CHILDREN. *Monographs of the Society for Research in Child Development, Volume IX, No. 3. Serial No. 39.*

By Charles E. Henry. Society for Research in Child Development, National Research Council, Washington. $1.50 (paper). xi + 71 pp. 1944.

This is the third in a series of reports of the Brush Foundation study of the growth and development of normal children. It is based on a study of almost 4,000 electroencephalograph records, many of which represent successive examinations of the same individuals over a period of years.

The aims of this study were to establish electroencephalographic standards, to explore the possible

correlations between EEG variables and other measurements of physical and mental development, and to determine the incidence of normal and questionably normal EEGs in a large group of presumably normal children.

It was found that there may be considerable variability in the form of individual growth curves. Some evidence of sex differences was secured. Attempts at correlations of alpha frequency and percent-time-delta with IQ and skeletal maturity were not significant. The incidence of abnormal records in this supposedly normal sample is discussed.

HEALTH AND DISEASE

FAMILIAL SUSCEPTIBILITY TO TUBERCULOSIS: *Its Importance as a Public Health Problem. Harvard University Monograph in Medicine and Public Health Number 5.*

By Ruth Rice Puffer. Harvard University Press, Cambridge, Mass. $2.00. x + 106 pp. 1944.

Although its mode of inheritance still remains to be worked out, the existence of a genetic basis of susceptibility to tuberculosis may be regarded as fully established by the studies of Kallmann and Reisner on the differences in the incidence of the disease in monozygotic and dizygotic twins, siblings, and parents, and by the extensive study of tuberculosis in Williamson County, Tennessee, which takes the central place in this monograph. The author uses, throughout, the ambiguous term "familial susceptibility" favored by the medical fraternity, but it is clear from context and data that nearly if not always it is genetic susceptibility that is meant, and not a common susceptibility due to shared familial environment.

The subject is introduced by a historical survey of the problem and a discussion of "familial" susceptibility in leprosy, rheumatic fever, poliomyelitis, and diabetes for comparative purposes, as well as of experimental tuberculosis susceptibility in guinea pig and rabbit strains. The four principal chapters deal with tuberculosis in siblings, in consorts, in parents, and in children of index cases. These summarize a great deal of interesting statistics. In the Williamson County study, death rates of siblings of index cases are higher than those of siblings of consorts of the index cases, the latter group serving as controls. Most siblings were adult and separated, so that this difference shows the effect of common heredity. Tuberculosis in consorts of active (sputum-positive) cases was higher than in those of sputum-negative cases. Manifest tuberculosis was found to be higher when the consort comes from a family with parental or sibling tuberculosis than when the consort comes from a non-tuberculous family (33.3% against 11.1%). It is especially important, if confirmed, that the death rate from tuberculosis of consorts is *less* than that of siblings of the index cases, in spite of the factor of current exposure in the consorts and of separation in the siblings. The data on tuberculosis in parents and children of index cases offer further confirmation of the importance of the genetic factor.

In one respect the treatment of the data in the discussion seems faulty. Measures of the significance of differences in the comparisons made are not given. In many cases the partition of the individuals into groups makes the numbers in the groups compared rather few, and there is little doubt that some of the comparisons, made between percentages, do not possess statistical significance. In a few places, recognition of this is evident, and once or twice the author refers to the calculation of a Standard Error. In the great majority of instances, however, the reader is left to his own conjectures or calculations. The difference, mentioned above, between the consorts from tuberculous and non-tuberculous families is, for example, of doubtful statistical significance ($P = .02–.05$). This methodological flaw considerably detracts from the merit of an otherwise fine piece of work.

In conclusion, the author applies her findings to the control of tuberculosis. She lays special emphasis on the value of searching for cases among siblings, parents, and children of persons known to have tuberculosis, irrespective of their present household. Past emphasis upon household exposure to tubercle bacilli has not been very effective in helping to locate fresh cases, although of course household associates of cases should continue to be examined as a matter of precaution. If these policies can be put generally into practice, we may expect a much higher proportion of early diagnoses, with all that that promises for eventual victory over one of man's most stubborn foes.

BENTLEY GLASS

A TEXTBOOK OF PATHOLOGY: *Pathologic Anatomy in Its Relation to the Causes, Pathogenesis, and Clinical Manifestations of Disease.*

By Robert Allan Moore. W. B. Saunders Company, Philadelphia and London. $10.00. xii + 1338 pp. 1944.

This tremendous volume contains a summary of almost every pathological condition listed in the Quarterly Cumulative Index Medicus. In this respect, it forms a complete catalogue of disease, and can be recommended as a reference book for the advanced student.

As a textbook of pathology, however, its very length and comprehensiveness serve as handicaps. Facts are given on so many unimportant subjects that important ones fail to be properly emphasized.

The format departs somewhat from the conventional method of presentation. The section on general pathology includes all diseases of known etiology and

is further subdivided into metabolic diseases, inflammations, and tumors. Diseases of unknown or obscure causes are described in the section on special pathology, under the organ in which they occur. As a reference book, therefore, there is no difficulty in locating desired information. As a textbook, however, the sequence of subjects will be rather complicated and difficult for the medical student to follow. Brief definitions of types of inflammation are given in a preliminary survey of this subject, but more comprehensive descriptions of infections are made according to portals of entry. Thus, many complicated diseases must be studied before the simpler pyogenic infections are completed; and there is no comprehensive view of many agents of disease with multiple portals of entry.

The volume is illustrated profusely. Many of the illustrations are excellent and serve not only as illustrations of pathological processes, but aid in the correlation of the pathological material with clinical observations. Illustrations of spontaneous and experimental diseases in lower animals are interesting; and charts and diagrams of the distribution of diseases and of other statistical data are especially helpful. It seems unfortunate, therefore, that with so much original material available for illustrations of pathological processes, large numbers of reprints from previously published material has been used. For instance, at least sixteen illustrations were reprinted from MacCallum's *Textbook of Pathology* alone.

The bibliography is voluminous. References are listed according to subjects and are further explained by complete titles. This is a distinct service to readers seeking specific information.

To reiterate, this volume will aid the student interested in rare and obscure diseases, and will serve as a helpful reference volume rather than as a conventional textbook of pathology.

ELLA H. OPPENHEIMER

HEALTH AND FIRST AID.

By Morris Fishbein and Leslie W. Irwin. Lyons and Carnahan, New York. $1.20. vi + 372 pp. 1944.

In Part I of this carefully prepared little volume, the authors have presented, in clear and non-technical fashion, an abundance of pertinent, scientifically accurate facts relating to personal hygiene in the preservation of health and the prevention of disease. Special emphasis is placed upon the most reliable methods of preventing the spread of common diseases, and in the most acceptable practices in caring for the body in everyday living. Such topics as sleep, fresh air, exercise, posture, fatigue, bathing, clothing, and diet, as related to the maintenance of good health, are discussed with authority and completeness. An excellent chapter has been included on the effects of alcohol and tobacco on the body. The methods and materials of first aid, and the important aspects of accident prevention have been presented in Part II of the volume. The latest methods (derived from the medical experience of the present war) for the treatment of shock and serious wounds have been thoroughly discussed. Each chapter is provided with a series of questions designed to test the reader's understanding of the material covered in that chapter. The work will serve well as a text-book for high school courses in hygiene or as a home reference book.

B. AUBREY SCHNEIDER

CONTAGIOUS DISEASES. *A Guide for Parents. Second Edition.*

By W. W. Bauer. Alfred A. Knopf, New York. $2.50. ix + 188 + iv pp. 1944.

In an authoritative, not to say authoritarian, manner, the author discusses for the layman—in particular, for mothers—what they should know about quarantine, home nursing, and the various contagious diseases their children are likely to contract. The complications of home nursing, as outlined, will probably terrify most mothers into insisting on hospitalization for everything. On the other hand, the author is clear and forthright as to the need for quarantine and the character it should assume in varying circumstances. The chapters on the individual diseases are excellent. Unnecessary fears are allayed; methods of prevention, if available, are described; symptoms and appropriate care are given in sufficient detail; and of course we are told in every case not to attempt home medication, but to call the doctor. The omission of Rocky Mountain spotted fever (tick fever, as it is generally called, at least in this area) is a bit surprising, since it is becoming commoner yearly, and mothers certainly show more concern about it than about some of the diseases that are included. All in all, however, this is a very instructive and useful handbook for the home, and its standard is high. It should also be useful in courses of hygiene.

BENTLEY GLASS

THE COMMON COLD *and How to Fight It.*

By Noah D. Fabricant. Ziff-Davis Publishing Company, Chicago and New York. $1.50. 107 pp. 1945.

Whatever can be said today about colds, plain and fancy, is said here in accurate, simple, and entertaining fashion. The author, an ear, nose, and throat specialist, discusses the respective merits of nose drops, vitamins, vaccines, alkalinizing, and sulfa drugs, and cautiously explores the possibilities of future research for overcoming this most frequent enemy of good

health. He fares afield into allergy and sinus and mastoid complications sufficiently to show their relations to the common cold. This brief volume must be rated high among popular presentations of medical subjects.

THE WOMAN ASKS THE DOCTOR.

By Emil Novak. The Williams & Wilkins Company, Baltimore. $1.50. ix + 130 pp. 1944.

In this slender book, a gynecologist has undertaken to answer in simple language the common questions women ask him. He has succeeded very well indeed. He includes a simple account of sex determination and of sex differentiation—in the former, he makes the error of attributing it to the potency of the Y chromosome. His chapter on the superstitition and folklore of menstruation is amusing and good medical history. The anatomy of the reproductive organs, the nature and significance of menstruation, the role of the endocrine glands, puberty, menopause, disorders of menstruation, causes of sterility, and cancer of reproductive organs or of the breast form the natural divisions of his subject. He concludes with a brief chapter on the sex life of women and on methods of contraception, in which he gives a qualified recommendation of the Ogino-Kraus "safe-period" theory. The standard of factual accuracy is high and the discussion clear. Women will find it most valuable, both for personal reference and as a help in the sex education of their daughters. It should be very useful as reading or text material in courses of hygiene for girls up through the elementary college level.

A NATIONAL HEALTH SERVICE *Presented by the Minister of Health and the Secretary of State for Scotland to Parliament by Command of His Majesty February 1944.*

Ministry of Health, Department of Health for Scotland. The Macmillan Company, New York. 75 cents (paper). 85 pp. 1944.

Any plan for complete national social security must, of necessity, include a comprehensive public health organization capable of rendering, free of charge, to each and every citizen of the nation, irrespective of economic status, age, sex, religion, or political affiliation, such medical care and advice as may be necessary for the maintenance of a healthy population and a high standard of living. The organization must provide, in addition to the present services of care for the insane, the quarantine and treatment of certain communicable diseases, school and home nursing, etc., every form of health care from the general treatment of diseases and disabilities in the home, to the more specialized branches of medicine and surgery obtainable only in the hospital or sanatorium. The structure and activities of just such an organization are presented in this report by the Minister of Health and the Secretary of State for Scotland.

The system herein outlined for Britain does not completely replace the present system of public health, but is organized around the firm foundations of the existing system. The organization provides sufficient flexibility for the incorporation of a wide variety of existing local health organizations and circumstances, and has attempted to reconcile the two extreme points of view regarding health care, i.e., individual medical care, and completely socialized medicine. A large share of the responsibility of instituting such a system will fall upon the shoulders of the central and local medical advisory boards, whose recommendations to the central or local health officers will be based on existing conditions and the needs of the population at the moment. The service will be financed partly by central and partly by local funds, to which will be added the contributions of some sort of social insurance.

Detailed discussions of (a) the existing health service of Britain, (b) earlier proposals for improving the service, (c) methods of administration of the service, (d) the remuneraion of general practitioners, and (e) the financing of the system, are appended.

B. AUBREY SCHNEIDER

FOOTE'S STATE BOARD QUESTIONS AND ANSWERS FOR NURSES. *Essay and Objective Types Compiled from Actual Examination Questions Given Throughout the Country by State Examining Boards. Twenty-third Edition, Revised.*

By Jimmie K. Bratton et al. (Editorial Panel). J. B. Lippincott Company, Philadelphia, London, and Montreal. $3.50. xliii + 1159 pp. 1945.

The present edition of this comprehensive handbook follows the general plan of the earlier editions, although numerous alterations and additions have been incorporated to bring the work completely up to date. Both the theoretical and practical phases of nursing are covered by a wide variety of objective and essay type tests. For some of the objective type tests, line drawings have been used, the answers being given on the following page. New materials have been added to cover the recent advances in the knowledge of certain drugs and vitamins, and a table of common poisons and their antidotes has been provided.

Introductory chapters by Sister M. Berenice on hints to nurses preparing for State Boards, and by M. Cordelia Cowan on the purpose and value of State Board Exams, provide enlightening orientation for students preparing for their first experience with State Boards. The cover flaps carry useful lists of medical combining forms, and prefixes and suffixes relating to medical terminologies.

For nurses who are planning to take State Board

Examinations, or who are planning to return to the profession following a period of activity in some other field, the work will undoubtedly continue to be an extremely popular aid.

B. AUBREY SCHNEIDER

AMERICAN MEDICAL PRACTICE IN THE PERSPECTIVES OF A CENTURY. *Studies of the New York Academy of Medicine Committee on Medicine and the Changing Order.*

By Bernhard J. Stern. The Commonwealth Fund, New York. $1.50. xvi + 156 pp. 1945.

During the last hundred years American economy has changed from a rural and agricultural to an urban and industrial type. In medicine, scientific elements more and more replaced crude empiricism. Specialization became one of the basic characteristics of medicine; and doctors have tended increasingly to settle in urban areas, where hospital facilities, indispensable for this new type of medicine, are of easier access. Medicine has tremendously improved, but within the same period the cost of medical care, reflecting increased expenses for education and apparatus, has gone to unprecedented heights.

All of these changes and the new problems which they create for physician and patient are described in Stern's small book, which might be called a primer of medical economics. Stern's style is clear, his material well chosen. The New York Academy of Medicine is to be congratulated for sponsoring this book, the first of a series which, we hope, will live up to the expectations created by this first volume.

The strictly historical parts of the book are short but excellent. They represent a type of medical historiography of which we are in dire need, especially in the field of American medical history, where the "romantic" type, as Galdston calls it in his interesting preface, still prevails. It fills the medical historian with a certain melancholy when he has to state that so far we have been provided with good American medical history mainly by "laymen" (Stern, Deutsch, Shryock). It is to be hoped that these examples will eventually have a salutary influence on those medical men who make such laudable efforts to trace the history of their science and profession.

ERWIN H. ACKERKNECHT

PATIENTS HAVE FAMILIES.

By Henry B. Richardson. The Commonwealth Fund, New York. $3.00. xx + 408 pp. 1945.

This exceedingly interesting book is devoted to an exposition of the abundant gaps in the understanding of individual patients brought about by inadequate appreciation of the fact that individual difficulties very often exist in a setting of family discord. The author has taken member by member, a few specific families who had visited at one time or another the out-patient departments of the New York Hospital, and out of these disjointed accounts of the separate members of each family, and by rather expert sleuthing on his own part and that of trained workers helping him, he has pieced together some very dramatic stories indeed. Not only this, but he has been able to penetrate behind the drama and to the motives at work, and so to help the individual patients to a relatively good adjustment, certainly to a better adjustment than ever would have been obtained by the piecemeal methods previously employed. The whole project, with the help of the Commonwealth Fund, was a joint effort with the Departments of Medicine and Psychiatry, the Macy Foundation, the Community Service Society, the Department of Public Health and Preventive Medicine, Cornell University Medical College, and the American Museum of Natural History,—the active work, I gather, having been performed by a small group of the Department of Medicine, Department of Psychiatry, and the Community Service Society. A great deal of work went into this project, and a kind of teamwork was fashioned that evidently grew in its effectiveness and understanding as the project went along. The method outlined in this book and exemplified in the records of the several families certainly presents an outstanding advance over the methods usually employed in our large hospitals, and a family clinic might very well be organized to take care of certain especially troublesome problems. Richardson, as an internist, has demonstrated a fine grasp of psychiatric principles of the more stable sort. One may suppose that he started with a considerable initial interest and developed his own insight as he went along. It is too bad that psychiatry and internal medicine have not more opportunity for just this sort of collaboration. It is an excellent book, to be read by every psychiatrist and internist.

THE STORY OF A HOSPITAL: *The Neurological Institute of New York 1909–1938.*

By Charles A. Elsberg. Paul B. Hoeber, New York and London. $3.50. xii + 174 pp.; 25 plates. 1944.

This little book is a straightforward chronicle of the evolution of one of the pioneer institutions of medical specialization in this country. Dwelling more on administrative and personal than on medical data, it is rather a contribution to hospital history than to medical history. Giving the typical pattern of incessant change and heroic struggle against material difficulties, Elsberg's book will be a valuable source for those who study the history of the hospital in the U. S.

VENTURES IN SCIENCE OF A COUNTRY SURGEON. *By Arthur E. Hertzler. Published by the author, Halstead, Kans.* 304 pp. 1944.

A sort of autobiographical addendum by the author of *The Horse and Buggy Doctor*, this volume is built around his ventures in medical science, clinical and otherwise. The remarkable thing is that so busy and successful a practitioner should have been able to find so much time and energy for research and writing. His story is written in a breezy, entertaining, unliterary style. But the effort to maintain a continuous flow of wit and erudition frequently is so obvious that it is irksome. There is at least one glaring error of attribution—the repeated ascription to Kant of Descartes' famous "I think, therefore I am." Some of the author's pet aversions are the American Medical Association, social workers, socialized medicine, full-time workers in science and education, that part of the American population east of the Alleghanies, philosophy, and Chaucer. These sometimes are assaulted with more spleen than logic, but always in an entertaining manner.

INTRODUCTION TO PARASITOLOGY *With Special Reference to the Parasites of Man. Seventh Edition.* *By Asa C. Chandler. John Wiley and Sons, New York; Chapman and Hall, London.* $5.00. x + 716 pp. 1944.

Without a doubt this is one of the most readable textbooks that is now available for an introductory course in parasitology. Chandler presents fundamental facts and principles of human parasitology in a clear, interesting manner, and has taken every opportunity to enliven the factual information with fascinating anecdotes and historical sidelights. Twenty-six years ago, when the author wrote the first edition, it was his wish to present the subject of parasitology in a manner to attract a wider group of readers than is usually attracted to the more conventional scientific textbook. It has been his belief that "scientific work flourishes best when intelligent public sentiment is behind it, and such sentiment is dependent upon capitalization of results already achieved, and a clear understanding of future goals." Although the present edition is a thoroughly scientific and scholarly treatise, it still retains the original freshness.

Usually, the subject of parasitology is restricted to the protozoa, helminths, and arthropods that cause or transmit disease. The author, however, has broadened the scope of his book to include the spirochetes, rickettsias, and filterable viruses. These have been included because they are frequently inadequately covered in bacteriology textbooks and are continually referred to in the discussion of arthropod vectors of disease.

The student is encouraged to consult the original literature by the author's practice of naming the investigators responsible for the more significant facts and opinions, and by the conveniently located complete references at the end of each chapter. The student's attention is also directed to lists of other books on parasitology, and to American and foreign journals that frequently publish articles on parasitology and related subjects. The author has the tendency to cite the more recent authority, if two investigators have demonstrated the same fact. For example, he refers to the work of Pipkins (1942) and not to that of Root (1921) in discussing the survival of *Endamoeba histolytica* within the house fly. When all of the authorities cannot be mentioned, it is, of course, debatable as to which should be excluded. Nevertheless, one must certainly commend the author for keeping up with the recent literature and for incorporating the significant findings in the present revision.

Laboratory techniques are not presented in great detail, so the book will not be found particularly suitable as a reference work for technicians. Nevertheless, all technicians concerned with parasitic diseases should certainly read the author's practical evaluations of diagnostic procedures and his emphatic plea for correct diagnoses. The following statement illustrates the author's stand on this matter, as well as his interesting style of writing: "As great danger lies in making a false positive diagnosis as a false negative one, for to inexperienced workers an ameba is an ameba, and often even epithelial cells and other objects are amebae. Many a patient, unfortunate enough to have an undiscriminating technician find an innocent *E. coli* or *Endolimax nana* in his stools, has had to submit to a course of treatment which was useless if not injurious to himself, and quite innocuous to the amebae. No doubt many patients with bacillary dysentery, having the numerous leucocytic cells diagnosed as amebae, have been sent to the grave or had their illness intensified by a toxic course of emetin which was entirely uncalled for."

The book is not as profusely illustrated as are some of the more recent textbooks on the same subject, but the three hundred odd figures that are presented have been well selected with but few exceptions, such as figure 52, on the life cycle of the malarial parasites, which pictures the sporozoites entering the red blood cells of man after inoculation by the mosquito. Although the author discusses the recent evidence against this direct penetration, his illustration is based upon the 1902 observations of Schaudinn that have never been confirmed.

Due to the clear presentation of facts about parasites and parasitic diseases, and above all, because of its ability to hold the interest of the reader, this edition should prove an excellent text for any course in parasitology where the subject is being presented to the student for the first time. It would seem to make little difference whether the course is being offered to undergraduate, graduate, medical, or veter-

inary students. Although the clinical symptoms of the various diseases are discussed, major emphasis is placed upon the biological aspects of parasitology. Regardless of the students' future application of parasitological knowledge, this biological foundation is essential for a clear understanding of the subject. More specialized information could be obtained by advanced courses and through the reading of the original literature.

M. M. BROOKE

INFECTIOUS ANEMIAS DUE TO BARTONELLA AND RELATED RED CELL PARASITES. *Transactions of the American Philosophical Society, New Series, Volume XXXIII, Part III.*

By David Weinman. The American Philosophical Society, Philadelphia. $1.25 (paper). Pp. 243–350; 1 plate. 1944.

"One of the few exclusively American and exclusively tropical diseases, human bartonellosis has become an object of major concern in western South America. Causing at times alarming epidemics with high mortality, it is also established in widespread endemic centers. In such endemic foci it has an important effect on the health of the population, and from these centers the possibility of dissemination constitutes an everpresent hazard."

With the above statement, Weinman begins his discussion of the public health aspects of human bartonellosis, or Carrion's disease. Although today it is an acute problem only in certain areas of South America, it should not be considered as inherently restricted to its present geographical boundaries. The recent epidemic in Colombia, which was previously considered outside the endemic area, has demonstrated the ability of the disease to occur in or spread to other localities. This observation, coupled with increased air transportation and improved highway systems, should emphasize the future potentialities of the disease. The public health hazards of this disease make it imperative for the medical profession of the Western Hemisphere to become better informed regarding its clinical manifestations, transmission, and control.

During the thirty-five years since Barton first described the organism that is now known as *Bartonella bacilliformis*, an extensive literature has appeared on this and related organisms. In the present monograph, the author presents the first comprehensive summary of the existing information. The following are a few of the generally accepted facts regarding human bartonellosis. It exhibits two rather diverse clinical pictures: (1) Oroya fever, a severe, infectious febrile anemia, and (2) verruga peruana, a benign skin eruption. Both of these conditions are produced by the same etiological agent, which is now believed to belong to the bacteria rather than to the protozoa. Man and monkeys are the only vertebrate hosts known to be susceptible. Transmission is by means of *Phlebotomus* flies; contact and congenital transmission play a possible but minor role. There is no effective therapeutic agent available, and control is primarily concerned with anti-Phlebotomus methods.

Although it is valuable to have our present knowledge of the subject accurately summarized and evaluated, the most important contribution of this scholarly monograph is the attention that it attracts to the problems that remain to be solved. Why does Oroya fever develop in one person, verruga peruana in another, and no apparent symptoms in still others? The mechanism of the great blood loss is unexplained. Is it due to a toxin as yet not demonstrated? How frequently does the *Bartonella* entirely disappear from the infected individual and how often does it just pass into a latent state? Is the resistance to reinfection always dependent upon a latent infection or is there a marked degree of residual immunity after the sterilization of the parasite within the host? Then of course there is the critical need for a therapeutic agent that is capable of curing the disease and entirely ridding the body of the parasite. In addition, there are many unanswered questions concerning the insect vector and its host-parasite relationships. What is the localization of the *Bartonella* within the *Phlebotomus* fly? And is congenital transmission to its offspring possible? The ecological requirements of the vector are not fully understood. The lack of this knowledge retards the development of more efficient control methods. These are but a few of the problems that are raised by Weinman. Although they are discussed with regard to a specific organism, many of them are concerned with fundamental problems common to several parasitic diseases. There are enough urgent problems on bartonellosis to challenge the best efforts of clinicians, bacteriologists, pathologists, immunologists, entomologists, and sanitary engineers for many years to come.

In addition to the discussion of *Bartonella* and human bartonellosis, the author also surveys the present knowledge of two closely related groups of organisms, the Haemobartonellae and the Eperythrozoa. Weinman has personally contributed much to our understanding of these organisms and is particularly well qualified to discuss their significance. He was responsible for observing the essential difference between *Bartonella bacilliformis* and the Bartonella-like organisms of lower animals which led to the establishing of the genus *Haemobartonella*. Although these organisms are only known to infect lower animals, they have many properties in common with the human *Bartonella*. They are therefore invaluable as laboratory infections for control experiments. Several references are made to the *Grahamella* organisms, but a thorough review is not given. Inasmuch as they closely resemble the other organisms

discussed, it would have been valuable if more information had been presented for the sake of comparison.

The monograph possesses an index, extensive bibliographies, and one plate. The plate pictures the morphology of *Bartonella bacilliformis, Haemobartonella microti,* and *Eperythrozoon coccoides.*

M. M. BROOKE

MICROBES THAT CRIPPLE.

By T. Arthur Turner, under the direction of Edward L. Compere. The National Society for Crippled Children, Inc., Elyria, Ohio. $2.50. xi + 241 pp. 1944.

Sufficiently accurate as to be authoritative, up-to-date and entertaining, but far from melodramatic, this book is of the sort seldom written and always needed. It presents problems factually in a straightforward manner. There is no eulogizing of the successes of medical science. There are chapters on the important crippling diseases or agents of diseases such as osteomyelitis, streptococci, tuberculosis, arthritis, virus infections, and syphilis. In each case the nature of the disease and its extent as a public health problem are considered, as well as something of its bacteriology, immunology, and epidemiology. There are also interesting and clear general discussions of infectious agents, of resistance to disease, and of recent advances in chemotherapy. The terrain, i.e., mainly the skeletal system, is described graphically. The concluding chapter on development of public health consciousness and modern services is excellent.

The book was apparently designed for persons concerned with rehabilitation work, teachers of crippled children, and others interested in problems of crippling diseases. However, it should be a valuable asset in the high school classroom, in elementary hygiene courses in college, and to all public health workers. It should be read by all intelligent people interested in community welfare.

It is easy to read. This slender volume is handsomely made; the type is large and clear and titles and subheadings are nicely set off. It is well written; rich, but not overly rich, in historical allusion; and at times it is humorous. Statistics are few and impressive. The numerous illustrations, some of which are in color, are particularly amusing and attractive and are mostly original drawings; they constitute a notable portion of the book. From the enormous amount of available material on the subjects considered, selection has been careful and well-balanced.

The index is adequate and the fairly extensive bibliography and fine 7-page glossary of terms should be helpful to the average reader.

HARRIETTE VERA

LABORATORY MANUAL FOR GENERAL BACTERIOLOGY.

By Department of Bacteriology and Immunology, University of Minnesota. W. B. Saunders Company, Philadelphia and London. $1.00 (paper). vi + 54 pp. 1945.

This manual was designed for use in an introductory course in general bacteriology. There are 29 exercises to be covered in 30 or 31 two-hour sessions of a quarter term or semester course. The scope is broad and includes exercises on such topics as soil and plant bacteriology, fungi, and water and milk analysis, as well as on pathogens and morphology. The selection of material for a first course in this field is always an extremely difficult matter, and teachers do disagree as to relative importance of subjects that could be included. There is a notable lack in this manual of any presentation of principles of disinfection and possibly there is undue emphasis on staining techniques and microscopy. A considerable number of exercises are devoted to pathogens, but there is none on anaerobic bacteria.

The directions seem to be clear, appropriate, and definite. There is a reasonable amount of explanatory material that should be of considerable help to the student new to the laboratory, and to the teacher as well.

HARRIETTE VERA

TRAVAUX DU LABORATOIRE DE MICROBIOLOGIE DE LA FACULTÉ DE PHARMACIE DE NANCY. *Fascicules XII (1939–1940–1941) and XIII (1942–1943–1944).*

Société d'Impressions Typographiques, Nancy. 112 pp. + 3 plates; 338 pp. + 10 plates. 1941; 1944.

BULLETIN DE L'ASSOCIATION DES DIPLÔMÉS DE MICROBIOLOGIE DE LA FACULTÉ DE PHARMACIE DE NANCY. *Numbers 24–25.*

Société d'Impressions Typographiques, Nancy. 76 pp. + 2 plates. 1942.

The two volumes of "Travaux" are reports of work carried on in France, during the war years, by Lasseur and his co-workers. The foreword of the earlier issue (XII) contains the statement that only part of the research accomplished is covered here. The destruction of cultures is mentioned in several places. The volume is, however, nicely printed on fine glazed paper. Most of the issue is taken up by a group of papers on agglutination and electric charge of dissociants of *Serratia, B. chlororaphis, B. lactis niger* and *B. mesentericus niger.* The second part, under the title of general microbiology, includes an article on the cultural characteristics of *B. mesentericus niger* and *B. lactis niger,* and two on conductibility of bacterial suspensions.

The later and larger volume, which is printed on very poor quality paper, states that the publication has been

urtailed due to circumstances and that the work has een, for the most part, recorded without discussion interpretation of results. Most of the number is evoted to the physiology of bacteria, to studies of hromogenesis in relation to the utilization of heavy etals and certain amino acids in culture media, or to lumination and irradiation of microorganisms. There re two articles on antigens and agglutination of *hromobacterium chocolatum*.

The "Bulletin" contains rather brief scientific papers n serum coloration, by Roy and Boutaric; on microbial ultiplication, by Regnier and Lory; and on a pneuatic micromanipulator and a study of *Leptospira* ranules, by Bessemans, Wittebolle, and Baert. here are testimonials to Beijerinck, Ramon, Gorini nd Weinberg, that to Weinberg being especially interesting because of his work on wound anaerobes during he first World War.

HARRIETTE VERA

ANIMAL NUTRITION

THE AVITAMINOSES. *The Chemical, Clinical and Pathological Aspects of the Vitamin Deficiency Diseases. Third Edition.* *By Walter H. Eddy and Gilbert Dalldorf. The Williams & Wilkins Company, Baltimore.* $4.50. xi + 438 pp.; 47 plates. 1944.

The new edition of this very excellent compilation has been "rearranged to sharpen the separation between the deficiency diseases and the chemical nature and functions of the vitamins," according to the authors in their preface. They have succeeded well in their task.

The book is divided into three main parts: Part I, The Vitamins; Part II, The Avitaminoses; Part III, Technical Methods of Vitamin Assay and Vitamin Values. The first part of the book includes an introductory chapter with an excellent table of "Postulated Vitamins," and another of "Some Vitamin Units and Reference Standards." This part then continues with a discussion of the chemical nature of the vitamins, their behavior and requirements. Then follow chapters on the nature and functions of the individual vitamins. Each of these chapters contains a brief historical introduction to the discovery of the vitamin, and then discusses its nature and its function.

Part II contains chapters discussing in detail each of the vitamin deficiencies, again including historical information. In these chapters a clear differentiation is made between the symptoms produced in experimental animals and those exhibited by humans. Excellent pictures illustrating deficiency symptoms are included in the book, as well as histological sections. The chapters have subdivisions which are valuable in organizing the information.

Part III has to do with vitamin assay methods. These chapters are brief and merely discuss the type of test used, with references to the original literature.

At the end of the book, there is a complete bibliography arranged according to chapters. Following the bibliography, there are tables showing the vitamin values of many of the common foods. A subject index follows this. The book is well written and contains a wealth of information, well organized and accurate. Much new data has been included. It will be an invaluable volume for nutritionists.

MARGUERITE M. SCHMIDT

HONEY AND YOUR HEALTH. *A Nutrimental, Medicinal and Historical Commentary. New and Revised Edition.* *By Bodog F. Beck and Doree Smedley. Robert M. McBride and Company, New York.* $3.00. 246 pp. 1944.

Few in this era of cane sugar realize what an important article in the diet honey was, up until modern times. Sacred for its nutritive and medicinal properties, the source of exuberance in the form of mead, honey was of paramount importance to the ancients. The authors of this book have collected an immense amount of entertaining lore about honey, and this forms the most valuable portion of *Honey and Your Health*. As to the relation of honey to health in modern times, they have not succeeded so well, largely because of the exuberant enthusiasm for their subject, perhaps inspired by one or another of the numerous ways of eating and drinking it for which they give recipes. The fact is that they are uncritical about the claims they make of its virtues. There is no doubt whatever that Americans (at least prior to the present sugar shortage) eat altogether too much cane-sugar for their welfare; nor is it disputable that honey (largely levulose and dextrose) is more rapidly assimilated. It is probably also valid to reason that honey is less likely to be injurious than sucrose because there is less temptation to over-indulge in it—satiation comes sooner. But it is surely going altogether too far to say that cane sugar is a drug and not a food, that it has calorie content that will provide heat and energy but that it does not provide nourishment; that "artificial [sic] sugars. . .oxidize violently in the human stomach. . .with an explosive effect that obliges the vital organs suddenly to work harder and faster"; that these artificial sugars and starch produce poisons and cause chronic fatigue, kidney and circulatory disorders, etc., while honey is not oxidized by the digestive tract and is nutritious, being converted to essential glycogen. This is twisting things with a vengeance. The authors would have made out a

much stronger case had they been more moderate in their claims.

A matter for surprise is the complete omission of any real account of the life of the honeybee or of its honey-making. To sum up, this book is an interesting and useful compilation of lore about honey, but biologically it is uncritical, not to say unsound, and omits the central biological story of honey almost entirely.

BENTLEY GLASS

BIOCHEMISTRY

PHYSICAL CHEMISTRY FOR STUDENTS OF BIOLOGY AND MEDICINE. *Third Edition, Second Printing.*

By David Ingersoll Hitchcock. Charles C. Thomas, Springfield and Baltimore. $3.50. xiii + 264 pp. 1940; 1944.

Clear in its treatment of topics selected for their applicability to biological problems, this text is outstanding in its field. The scope of the book is purposely limited largely to a consideration of the physical chemistry of dilute aqueous solutions, since, as most will agree, an understanding of the laws of dilute solutions is fundamental to that of newer developments. One touchstone of the excellence of such books as this is their treatment of osmosis. In the present case this leaves little to be desired, as it is clear, concise, and accurate. The treatment of electrochemistry has been expanded and rewritten, and now forms a sequence of six chapters. These are followed by chapters on colloid chemistry, membrane equilibrium and potentials, equilibria in blood, reaction velocity and enzyme action, and transformations of energy. These topics are all discussed in merely introductory fashion. The use of mathematics is held to a minimum throughout. At the end of the book is a series of 20 laboratory experiments, eight of these being newly added exercises in electrochemistry.

Many of the fields of newer importance to biology are not included. There is, for example, no discussion of the nature of the chemical bond in the light of recent theory, of rheological phenomena, of x-ray diffraction analysis and the structure of proteins, of absorption spectra, of tactoids, etc. The entire field of the effects of radiation upon chemical structure, so important in modern biology, is ignored. These remarks are not intended as criticism of a most valuable introductory text. It is, however, worth pointing out that the original selection from the vast field of physical chemistry of those topics most important to biologists must be subject to change as the trends of modern biology themselves alter.

BENTLEY GLASS

UNTERSUCHUNGEN ÜBER AMINOSÄUREN, POLYPEPTIDE UND PROTEINE. *Volume 1, 1899–1906. Volume 2, 1907–1919.*

By Emil Fischer. Volume 2 edited by M. Bergmann. (Julius Springer, Berlin); J. W. Edwards, Ann Arbor, Michigan. $38.00. x + 770 pp. (vol. 1); ix + 922 pp. (vol. 2). (1906; 1923); 1944.

In these two volumes are collected the 160 publications of Emil Fischer and his students on the chemistry of proteins and products derived from them. The experimental work that is recorded here forms the foundation for most of our knowledge of the organic chemistry of protein molecules.

Fischer's teacher, Von Baeyer, was engrossed in studying the structure and syntheses of dyes; but Fischer's interest turned to those organic compounds which constitute living matter, and he devoted his life to the study of sugars, proteins, purines, and the steric arrangements of these molecules. The scope and importance of his contributions in these fields are unique in the history of chemistry. The reports of his research on sugars fill two additional volumes—and this work is fully as important and perhaps better known than his contributions on proteins. Another volume is required to collect the results of his studies on purines.

Individual articles from the collected protein papers are not in themselves impressive. Each one deals concisely with some specific preparation—the digestion of an albumen; the isolation of an amino acid from such a digest; a chemical synthesis of an amino acid; the resolution of this amino acid into its optically active forms; the combination of two or more amino acids into polypeptides; etc. Consequently in going through the titles and even in reading the articles, one is hardly aware that here proline and hydroxyproline are being isolated for the first time, that the first chemical synthesis of serine is being reported, that racemic mixtures of amino acids are being resolved for the first time into their active antipodes, or that a polypeptide of 18 amino acid units (the most successful attempt at *in vitro* protein synthesis yet achieved) has been accomplished.

In all this work there is nothing really revolutionary, nor any dramatic discovery. Yet, when the individual contributions are reviewed as a whole, the epic nature of the work stands out, and one can see the aggressive and logical attack that Fischer made on the problem with all the organic and biological techniques inherited from the previous generation of famous German chemists.

There is included, with these experimental publications, the manuscript of an address on protein chemistry given in 1906 before the Deutschen Chemischen Gesellschaft, which is of interest because it so clearly outlines the exact status of the progress that had been made up to that time.

In his writings, Fischer used a style which is more modest, direct, and concise than is often found in German scientific literature. For this reason the articles are easy to read or translate, as is evidenced by their popularity with graduate students taking language examinations.

ROBERT E. EAKIN

A LABORATORY MANUAL OF PHYSIOLOGICAL CHEMISTRY. *Fifth Edition.*

By D. Wright Wilson. The Williams & Wilkins Company, Baltimore. $2.50. 269 pp. 1944.

This teaching manual contains many fine laboratory experiments in physiological chemistry. Part I of the manual is considered "Introduction." It contains procedures for the determination of inorganic constituents, for the preparation of standard acids and alkalis, for the determination of electrolytic dissociation, and for the study of colloids, alcohols, aldehydes and esters, carbohydrates, proteins, and fats. The directions for these are clearly written, and some include a brief discussion of the theory of the procedure. The tests which have been selected are a review of procedures in other courses, but with particular emphasis on those associated with biological material.

Part II of the manual contains procedures on body tissues and fluids. Here we find a study of saliva, gastric and pancreatic juice, milk, blood—general and qualitative, bone, muscle, the cell nucleus, bile, urine—qualitative, normal and pathological, and quantitative. There are also included diets for the study of dietary deficiencies, a table of logarithms, and an abridged table of atomic weights. As in part I, the experiments are well chosen, directions are clearly written, and there is some discussion of theory. References to the original literature are also included. Directions for making various solutions are included in footnotes.

The printed matter is all on the left hand pages, the right hand pages being left blank for recording the results. The manual is one which could be adapted for use in any course of biochemistry. It has many experiments, use of which can be limited as the needs of the students demand.

MARGUERITE M. SCHMIDT

FUNDAMENTALS OF INORGANIC, ORGANIC AND BIOLOGICAL CHEMISTRY, *with pamphlet, An Outline for Teachers.*

By Joseph I. Routh. W. B. Saunders Company, Philadelphia and London. $2.25. vii + 274 pp. 1945.

This text has been written primarily for use in teaching chemistry to nurses. As the title implies, it includes the fundamentals of inorganic, organic, and biological chemistry. In itself, the text is clearly written and well expressed. However, in his effort to include only the absolute essentials in inorganic and organic chemistry for an understanding of biological chemistry, the author has omitted some of the essentials necessary for accurate information, particularly in inorganic material. The discussion of valence and oxidation-reduction is not well done. The chapters on biological chemistry are full, containing much information—more than is necessary for a nursing course. While one may agree with the author, in his preface, that students respond better to biological chemistry, and that such information is more necessary for nurses, yet for an adequate understanding a more thorough background than Routh gives is necessary. The book might be a satisfactory text for a group who have had a year of thoroughly taught inorganic chemistry. The teacher's outline is adequate for a novice, and will aid in outlining the work for her.

MARGUERITE M. SCHMIDT

OUTLINE OF THE AMINO ACIDS AND PROTEINS.

Edited by Melville Sahyun. Reinhold Publishing Corporation, New York. $4.00. 251 pp. 1944.

At the present time there is great interest in proteins and amino acids. This book is a concise, readable outline of this field, now finding application in practically all branches of biology. Clarity and simplicity have been achieved by touching lightly on the complicated theoretical and argumentative aspects of the subject. These have been covered by the larger treatises mentioned in the adequate bibliographies at the end of each chapter.

The twelve chapters cover the whole field of protein chemistry, from the account of the discovery of the amino acids to the metabolic and nutritious aspects of amino acids and proteins. It contains chapters on the structure and hydrolysis of proteins as well as on their synthesis and methods of analysis. For biologists who are not specializing in this field, the book represents the most efficient means of bringing their knowledge up to date. It will also be of value, however, to those who wish to obtain the necessary background for intensive study. It is a simple, readable presentation of the essential and fundamental aspects of the subject.

FRANK H. J. FIGGE

ENERGY RELATIONSHIPS IN ENZYME REACTIONS. *Annals of the New York Academy of Sciences Volume XLV, Art. 9.*

By Joseph S. Fruton, Eric G. Ball, Max Bergmann, Herman M. Kalckar, Otto Meyerhof, and Carl V. Smythe. New York Academy of Sciences, New York. $1.00 (paper). Pp. 357–436. 1944.

This is a series of papers which resulted from a conference on energy relationships in enzyme reactions held by the section in physics and chemistry of the New York Academy of Sciences in February, 1944. At first sight, the title of this symposium would make it appear that this was a premature discussion of a subject about which little is known. The papers show, however, that some advancement has been made, "so that probing fingers are already beginning to search for the mechanism whereby this energy release is geared to perform the functions essential to life."

The introduction is a concise summary of our present knowledge of enzymes. The most provocative and at the same time most informative paper in the series is that by Ball, on the energy relationships of the oxidative enzymes. The discussion of energy relationships in glycolysis and phosphorylation is concerned chiefly with the complicated subject of muscle metabolism and is marred by the author's egotism. The significance of coupled reactions for the enzymatic hydrolysis and synthesis of proteins, discussed by Bergmann and Fruton, is both timely and excellent. "From the experimental results and the speculative considerations which have been presented in this paper, it follows that future progress on the problem of the biosynthesis of proteins and peptides depends on a more intensive investigation of coupled reactions with well-defined enzyme systems and with substrates of known structure." The final paper, on some enzyme reactions of sulfur compounds, is a speculative account of the metabolism of methionine, cysteine, glutathione, and other sulfur compounds.

FRANK H. J. FIGGE

THE CHEMISTRY AND TECHNOLOGY OF FOOD AND FOOD PRODUCTS. *Volume II.*

Edited by Morris B. Jacobs. Interscience Publishers, New York. $10.50 (both volumes—$19.00). xx + 890 pp. 1944.

This is the second volume containing the concluding sections of an exhaustive treatment of the chemistry and technology of food and food products. The first volume was reviewed in the March, 1945, number of this journal (20: 101).

Part III is a general discussion of large scale unit operations and processes. The general facts relating to sanitation and quality control of food are adequately covered in six chapters in Part IV. This includes a consideration of Governmental agencies and regulations, food grading and food machines, detergents and sanitation, the control of insects, molds, and rodents. Part V is a comprehensive survey of the general and specific methods of preserving and packaging food. The final section deals with the production of cereal grain products, sugar, confectionery, jams, and jellies, milk and milk products, meat, and alcoholic and non-alcoholic beverages. The methods used to treat water which is to be used for drinking and manufacturing purposes are also described in detail.

In general this will be a useful reference book for food technologists of all classes. Specialists in this field may occasionally have to consult the references cited for specific details. The value of such a reference work depends on how carefully and thoroughly the subject index has been prepared. Extensive use of the index, which covers 55 pages of fine print, has convinced the reviewer that this is one of the best features of the book.

FRANK H. J. FIGGE

PSYCHOLOGY AND ANIMAL BEHAVIOR

HISTORY OF PSYCHOLOGY *From the Standpoint of a Thomist.*

By Robert Edward Brennan. The Macmillan Company, New York. $3.00. xvi + 277 pp. 1945.

The author has stated what may be called a four-fold purpose for his book in his Prologue: first, to correct false notions of what psychology is by showing that its real business is "the study of human nature" rather than either mind reading or psychoanalyzing; second, "to relate the tale of psychology in a strictly traditional manner"—which no other book, to his knowledge, has attempted to do—". . .an interpretation or a point of view that Aristotle or Thomas Aquinas might have given us, were they able to look back like ourselves, over the perspective of nearly three thousand years' development"; third, to show that the "story of psychology has a philosophic as well as a scientific background—far more of a philosophic one," which most chronicles of today hardly show "due to a positivistic prejudice," that is, "to a conscious or unconscious attitude, on the part of certain scientists, which presumes that philosophy has nothing to contribute to the field of psychology"; and fourth, to declare his dissatisfaction with the way in which the story of psychology has been told heretofore, and to point out the possibility of a new way of approaching its contents, although recognizing the inadequacy unavoidable in so short a space. He claims "that all the factual data that have accumulated in the laboratories and clinical chambers over the past half century, can be placed only in one tradition, so far as their philosophic significance is concerned, the tradition of Aristotle and Thomas Aquinas." This he attempts to prove. He admits that in the effort to be brief he may have fallen into the vice of obscurity.

From the two ways of approaching the study of man

he scientific and the philosophic, he arrives at a definition of psychology which embodies the two, as "the study of acts, powers, habits, and nature of man" rom the phenomenal, or "empiriological," and the nouenal, or "ontological," standpoints.

In his chapter on "The Thomistic Synthesis" we ave what, judging from the subtitle of the book, the uthor regards as its heart. The synthesis of Aquinas e considers remarkable on two scores: first, he "linked ogether all the scattered and often fragmentary inights into reality" of the preceding thinkers into an rganic whole; and, secondly, he was able to add to the ctual content of philosophic knowledge by his own rofound analyses." Moreover, Aquinas "was able to erfect the method of the schools, by consecrating to he service of truth the dialectical skill which the rationalists so highly praised and which the mystics so roundly condemned." In a way analogous to that of Aristotle mentioned above, "Aquinas distinguished human from divine wisdom at the same time that he showed the basic continuity of philosophic and theological experience in the total content of man's knowledge."

In his presentation of "The Core of the Thomistic Psychology," the author should be quoted, perhaps, rather than paraphrased: "What Aquinas did for philosophy in general he did for psychology in particular. Here the thing that characterizes his work is the new spirit with which he addressed himself to the task of analyzing the nature of man in the light of Aristotle's hylomorphic doctrine. In his psychology we can discover Platonic elements as well as the substantial features that were Aristotelian. With a like sense of impartiality, he sought to discover the possible uses of the Augustinian doctrines that were common in his day. Nor did he disdain to consult the Jewish and Arabian commentators for the solution of his problems. He worked on the principle that all knowledge and all truth, whatever its source, is capable of harmonious adjustment." "The solution of the major issues of psychology in Aquinas's day demanded that the body-soul problem be given first place. Aristotle had merely declared that the true solution was so obvious as to make clarification superfluous, beyond a mere statement of the fact of the substantial union of these two basic principles of human nature. Not so Aquinas! He insists on a demonstration; and his emphasis on the necessity of proving this central assumption in the Aristotelian psychology, shows, I should say, that he was much closer to the temper of modern psychology and more appreciative of its needs than was Aristotle himself. The question here, to be sure, is merely one of focus" (pp. 62–63).

There are three great trends of thought that have dominated the historical development of psychology from the days of the Greeks: the tradition of Democritus, which is the spirit of materialism—holding that only matter is a valid object of study; the idealistic one of Plato—that only mind is a valid object of knowledge, because only mind is real; and the Aristotelian, in which both mind and matter are knowable, because both are real, and man is knowable because he is a form immersed in matter, a form "in which both body and soul are joined as essential elements."

It is Aristotle's tradition "which is emerging today with new depth and vigor when confronted with the impartial evidence of the investigators. More than ever before, we realize now that the observed facts of consciousness must be correlated with the physiology of human life if they are to be correctly understood." Furthermore, "that the mechanistic concept of the soul as a property of cortical substances or the Cartesian notion of it as an immaterial entity whose whole nature is to think, must be ruled out." Such ideas "do not fit the evidence." If any solid advance is to be made in the field of psychophysical analysis, it must be on the principle that "the soul of man, like the first actuality of all other cosmic creatures, is a form united substantially with matter." "This being the case, psychology cannot limit itself to the phenomena of consciousness, but must extend its technical observation to the *whole man*" (pp. 252–3).

The author's treatment of the numerous "traditions" and schools, from the days of Aquinas down to the present, is necessarily brief and concise. He tries to evaluate them in a fair-minded manner, although, of course, we should hardly expect the followers of any one school to be wholly satisfied with the fulness of treatment, even if they feel that the evaluation is fair.

The book, though short, clearly and interestingly written, and one which fills a special need in the field of psychology, requires of the reader a good background in philosophy and psychology for an adequate appreciation of the full story of psychlogy which it presents. It should serve well for both a pre-view and a review for those with the foundations; but it could hardly be considered as a primer. Supplemented with selected readings from the many sources drawn upon by the author and listed in the Bibliographical Index and the Bibliography, it might well be used as a textbook in undergraduate classes. For this purpose, however, a bibliography of detailed selections from the sources might well be provided for each chapter, together with a list of questions to guide the student in his readings.

C. Hart Westbrook

Psychology.

By Floyd C. Dockeray. Prentice-Hall, Inc., New York. $3.00. xiv + 504 pp. 1944.

This is the fifth printing of the popular textbook first printed in April, 1942. The author's purpose for this text has been "to help the student to understand human behavior, and, second, to encourage him to practice

those principles which apply to his own case." Recently authors have had to meet this problem, as psychology has grown more popular and teachers have tried to make it more interesting to pupils by the use of applications to their everyday life, while at the same time acquainting them with the known facts of psychological research and investigation, the methods used, and the "development of generalizations or principles." How can one present the subject as a scientific discipline yet make it "interesting," show its practical applications, and, perhaps, give students an opportunity to study themselves in an elementary way and understand the meaning and the significance of the terms, symptoms, and disorders found in abnormal psychology, and so widely bandied about in the press and popular magazines? It seems that the author has stood by his chief aim of presenting psychology as a scientific subject very well, and has given adequate attention to the other demands without going too far towards overpopularization. His text has already been widely used and highly spoken of.

C. HART WESTBROOK

THE BASIC TEACHINGS OF THE GREAT PSYCHOLOGISTS. *By S. Stansfeld Sargent. The New Home Library, New York.* 69 cents. xiv + 346 pp. 1944.

This book surveys the entire field of psychology aspect by aspect, from a historical point of view. It is a sound and useful reference or introduction, although too condensed to permit real insight into any one phase. For example, Freudianism gets approximately six pages, along with some scattered references, and the work of G. Stanley Hall and J. B. Watson two or three pages between them. In the generally good chapter on the effect of heredity and environment, there are a number of erroneous or questionable statements, such as the one that "only two mental abnormalities definitely are inherited—feeblemindedness and Huntington's chorea." For a bird's eye view of historical psychology, the book will prove useful for students and laymen. Its exposition is clear and simple, although its style is rather undistinguished. Its organization is logical and effective.

DVORINE COLOR PERCEPTION TESTING CHARTS. *Volume 1.*

DVORINE COLOR PERCEPTION TRAINING CHARTS. *Volume 2.*

By Israel Dvorine. Published by the Author, Baltimore (2328 Eutaw Place). $25.00. Volume 1, 1 rotating disk and 60 color charts; Volume 2, 4 color plates and 70 color charts; instruction manual (paper), 24 pp. 1944.

The charts included in these two volumes follow the conventional plan which was introduced by Stilling, adopted by Ishihara, and extended in the Test published by the American Optical Company. Since the reading of numbers or letters on charts of the Stilling type is dependent on a differential in brightness between the colors on a chart and not on a differential in hue, such chart-tests cannot be considered reliable. It is true that a change in brightness differentials in the vision of a "color-blind" person is often associated with a change in hue-differential; but some persons who fail the chart-tests have no difficulty in distinguishing hues, even the hues of the numbers they cannot read; while some who can read the numbers on the charts are seriously "color-blind."

It has often been pointed out that the inclusion in the sets of charts which the "color-blind" person can read, but which persons with "normal" color vision cannot read, indicates clearly that the reading is not based on differences in hues, but is based on differences in brightness. Some "color-blind" individuals can read the charts which "normals" are supposed to read, because the luminosity-thresholds of the critical hues have altered so much that the numbers, which for the "normals" are brighter than the surrounding spots (constituting the background), are for them darker than the background; and vice versa, for other charts.

Two features of Dvorine's volumes merit attention. The first feature is inclusion of materials for testing the accuracy of naming of colors; and color naming is undoubtedly important. In highway traffic, for example, the light which is seen by drivers with "normal" color-vision as green, may be seen by a color-blind driver as a slightly greenish blue; if the driver, however, has learned to call the color "green" he knows that it means "go." Similarly, if the light which "normals" see as red is called "red" by the "color-blind" driver, although he may see it actually as orange, he knows the light means "stop," and is no danger. A color-blind driver who names the lights as do persons of "normal" color-vision may even be a safer driver than are many "normals."

There are two difficulties involved in Dvorine's charts for training color perception. One is the omission of the names by which the colors should be called. It is well known that persons of "normal" color-vision differ from one another in the names they apply to colors, especially to colors of low saturation. Hence the validity of the training depends upon the names applied by the particular trainer. The second difficulty is that a really color-blind person would be trained to call the colors by names of colors he does not actually see; this would confuse him and cause him trouble in traffic.

The second important feature of Dvorine's presentation is the assumption that color-vision can be improved by training. We admit that color-naming can be improved by training; but that the fundamental

features of so-called "color-blindness" can be modified by training is merely an hypothesis. We know that a "color-blind" person can learn all the charts in a set; calling off the numbers correctly, although remaining unable actually to see the numbers on many of the charts. One young man memorized all the charts in the A. O. C. set, and then admitted that there were many numbers which he could not see. After a course of treatment with Vitamin-A and cobra venom, his color perception improved greatly.

That training may produce a slight improvement initially, due to adjustment to the test situation, assisted by better attention, is true; but beyond that point the usefulness of training in color perception is conjectural. However, Dvorine is to be commended for publishing the first set of charts by the use of which his theory can be experimentally tested.

KNIGHT DUNLAP

THE PSYCHOLOGY OF DIET AND NUTRITION.
By Lowell S. Selling and Mary Anna S. Ferraro. W. W. Norton and Company, Inc., New York. $2.75. 192 pp. 1945.

It is somewhat surprising that a scientific psychology has not grown up about the problems of food and eating. The psychology of sex is a highly developed field, yet food and the need for eating, along with hunger and appetite, are more omnipresent than sexual motivation. Why people form improper eating habits and why they do not eat suitable diets when these are available are questions that have assumed double importance in wartime. The nutritionist can prescribe an adequate or an optimum diet, but to make people follow it is a psychological problem of no mean order. *The Psychology of Diet and Nutrition*, written jointly by a psychiatrist and a dietitian, is an excellent preliminary approach to the study of these difficulties. The topics considered are: The Basic Psychology of Nutrition; Food Habits, Fads, Customs, and Aversions; Children's Feeding Problems; Feeding Problems Due to Psychological Maladjustments; The Dietary Significance of Mental Disorders; Food and Nutrition as They Affect the Home; Mental-Hygiene Technique Applied to the Feeding of Large Groups; The Special Psychology of the Patient on a Diet; The Effect of Improper Diet on Behavior and Personality; and Educating the Consumer.

These chapters are excellent, singly or as a whole. They are logically organized and clearly discussed, full of interest and helpful suggestion. There is an index. The volume will be of primary interest to nutritionists, doctors, psychiatrists, and psychologists, but it may also be recommended without reserve to the layman, the mother, indeed to all those concerned with provisioning or the preparation of food.

BENTLEY GLASS

HANDBOOK OF INDUSTRIAL PSYCHOLOGY.
By May Smith. Philosophical Library, New York. $5.00. 304 pp. 1944.

This book should appeal to persons in any walk of life who have contacts with those who live and work in industry. And now that industrial reconversion is upon us as well as the vocational guidance and rehabilitation of our returning service men and women, persons who are in applied psychology and those preparing to enter upon it, social workers, school psychologists, and medical practitioners, particularly, will find it useful. Those in industrial medicine, will find this an informing, stimulating, and interesting book.

The chapters deal with: Pioneer Work; Fatigue in Industry; The Environment (General and Material—Light, Temperature, Noise, Hours; Psychological—The Group, Mental Atmosphere, People in Authority); Finding the Job for the Person and the Person for the Job; Time and Motion Study; Temperaments, particularly the Nervous—Must the Nervous Person Break Down?; Why We Work—Some Symptoms of Something Wrong—Are Accidents Accidental?—Breakages—Grievances—Misfits—Sickness; Measures of Human Well-being (Absence through Sickness—Labor Wastages); General Hints in Methods of Investigating; Conclusion.

The interplay of the workers' psychological factors with the physical and environmental factors is especially well presented in the chapters entitled "Temperaments, Particularly the Nervous" and "Why We Work," on the basis of actual and recent industrial studies. Physicians, as well as foremen, managers, and industrialists will find these facts and principles and motivations to work significant. The author herself conducted the investigations of the nervous temperament and its influence upon efficiency and sickness.

The data used and interpreted are drawn mainly from the researches and investigations begun by the British Health of Munitions Workers' Committee and its successor, in 1919, the Industrial Fatigue (later, Health) Research Board, in connection with World War I's problems, and continued in many industries through the years of peace down into those of World War II. The Medical Research Council's Special Reports, the Journal of the National Institute of Industrial Psychology, and other journals have supplemented the earlier reports. And for these reasons the data are especially valuable and suggestive. Comparatively few tables are used, and they are short. Such data as are given are interpreted for the reader in the text itself in terms of percentages and hence are quickly and easily grasped. The book is a scientific production presenting scientifically planned and conducted studies, but is not technically dry.

The individual in his group, and at his job, is investigated or experimented with in such a way as to enable doctors, industrial managers, or others, to study

him as a whole human being in his social and his objective or work environment. And since certain of the mechanical factors can be regulated and studied very objectively and accurately, the part which these play in the psychosomatic situation can be more readily determined. This phase of applied psychology is rapidly growing in its exactness and value, and laymen, including the industrial and office workers themselves, should become familiar with the findings and their practical help.

C. HART WESTBROOK

PRINCIPLES AND PRACTICE OF REHABILITATION. *By John Eisele Davis. A. S. Barnes and Company, New York.* $3.00. xxi + 211 pp. 1943.

This is a presentation in a rather didactic, pedantic way of the general topic of rehabilitation as applied particularly to psychoses and psychoneuroses. There are fairly adequate chapters on diagnostic criteria, psychological approach to the problem, interest and effort theories, modern methods, therapeutic objectives, handicraft, education and art, and there is a great deal of common sense observation and deduction exhibited in these chapters. Nevertheless, the book is hard to read. The actual literary style is exceedingly difficult, at times so much so that the meaning is quite unclear. Furthermore, it is laden down with references in the text itself to what various physicians of considerable standing have thought about all kinds of problems having any bearing at all on the final problem of rehabilitation, so that one is lost in the details of accessory material. It seems that the principles and practice of rehabilitation ought to be reducible to a much clearer and much simpler statement. Nevertheless, practitioners of rehabilitation may gain some help from the book.

WENDELL MUNCIE

SOLDIER TO CIVILIAN. *Problems of Readjustment. By George K. Pratt. Foreword by George S. Stevenson. Whittlesey House, McGraw-Hill Book Company, New York and London.* $2.50. xii + 233 pp. 1944.

This is a very sensible account of the problems confronting the soldier who returns to civilian life, and conversely of those civilians who have a soldier returning to them, in the home, in industry, and in society generally. The problem of adjustment to the Army and the reverse problem are both very well presented, in terms which anybody can understand, and devoid of that sloppy sentimentality and scarehead stuff which has characterized a great deal of writing on the subject in recent months. The author has had extensive experience, both as an officer in World War I and in veterans' work and general mental hygiene activity, is thoroughly qualified to present the subject, and has done an excellent job.

WENDELL MUNCIE

PERSONAL MENTAL HYGIENE. *By Dom Thomas Verner Moore. Grune and Stratton, New York.* $4.00. vii + 331 pp. 1944.

A proper addition to the title of this book would be "for Catholics," since the author is a well known priest as well as psychologist and psychiatrist. The book has been passed through official Catholic censorship and contains statements and attitudes which would find general approval only in a Catholic society. This attitude is evident throughout in the appeal to a type of self-discipline to overcome various emotional difficulties, depression, anxiety, scrupulosity, etc. It reaches rather absurd proportions when the author takes Oliver Goldsmith to task, pointing out that poor Oliver might have led a happier life if he had only been less impulsive and had kept himself under better control in several ways. What the likely effect of this would have been on Oliver's poetry isn't noted. One may suppose also that Father Moore would find the same fault with Mozart. Why couldn't Mozart be satisfied in the services of the Archbishop of Salzburg, as his father so often implored him to be? In the light of history it appears completely unimportant that Goldsmith and Mozart were personally unhappy. Much more important is the likely fact that out of their unhappiness there developed a creative urge which has rarely been equalled.

Such attitudes noted above reach their apotheosis, however, in the chapter on "The Mental Hygiene of the Home." It takes a very special kind of bias to make such statements as the following: "Morality needs no customs for the source of its principles. Were all history destroyed, the fundamental relations of man to man in the existing social order would supply the material from which reason sitting in judgment on conduct would derive the necessary natural principles of the moral order. And action in accordance with these principles would constitute the essence of sound mental hygiene." Also, "To plan a marriage without children is in itself a crime. It is true that marriage is essentially a pact of perfect friendship, and because of this fact circumstances may arise that would justify a marriage when children would not be possible. But speaking of ordinary conditions a marriage without children is not to be contemplated. Mechanical methods of contraception are inherently wrong, and one is not allowed to practice the biological method of rhythm merely to avoid the burden of children." Also, "Divorce is always a tragedy," and concerning the "disintegration of family life," the following: "The stability of the family in the Middle Ages rested

to a large extent (1) on the law of charity governing the relations between parents and children, (2) on the Catholic doctrine of the indissolubility of the marriage tie, and (3) on the general concepts of duties and obligations existing between all members of the social order. With the advent of the Reformation, there extended into all spheres of life a tendency to deny the existence of authority and throw aside the ancient regard for all obligations and so exert the supremacy of the individual and his right to satisfy his personal desires. After ecclesiastical authority had been called in question, it was only natural that the existence of civil authority should be denied. And if there is no such thing as church or state authority, parental authority is easily disregarded." It need only be stated that these concepts will find general acceptance only in a Catholic society.

The most disturbing part of the book, however, is the revelation of the author's attitude toward his own profession, psychology. "We are confronted with the hopelessness of psychology. Religion alone can here open the way to sublimation that will transcend all the difficulties of life and the final failure of death. If mankind is to establish in society a stable mental peace it must come about by the realization that all nations and peoples constitute one single social order, that in this social order God is the infinite intelligence in a universe of intelligent beings. . .that every human being has a work to do in the world and when it is done he will attain to the knowledge of truth. . . ." One may wonder why such an intelligent attitude toward a world order and world peace has to be labelled as a religious concept. Father Moore apparently operates in a field of psychology which he conceives as a small area operating under a superstate of religion. It is not likely that this view will receive general approval from professional psychologists.

Actually, there is in this book a great deal of common sense advice that is of practical value. The appeal to the exercise of personal discipline is valuable, but would be much more valuable still if the author could have avoided the mistake of assuming that the understanding of the emotional life as practised in the best psychiatric circles is the equivalent of a ticket of admission to unlicensed emotional expression. Most of us feel that discipline and control are best established only in a setting of understanding.

WENDELL MUNCIE

MENTAL ABNORMALITY AND CRIME. *Introductory Essays. English Studies in Criminal Science, Volume II. Edited by L. Radzinowicz and J. W. C. Turner. Prefaced by P. H. Winfield. Macmillan and Company, London.* $3.75. xxiv + 316 pp. 1944.

This is a compendium written, at the suggestion of members of the Law School of Cambridge University, by outstanding specialists in psychiatry and criminology in Great Britain. There are thirteen chapters with almost as many authors, each contributing a little monograph in itself. The titles of the chapters indicate the scope of the work—mental variations and criminal behavior, psychoses and criminal responsibility, psychoneurosis and criminal behavior, mental deficiency, psychopathic constitution, functional nervous disorders after injury, physical factors, alcoholism, sexual offenders, juvenile delinquency, reaction to military life and criminal behavior, the diagnosis and treatment of delinquency, and finally a report on the work of the Exeter Child Guidance Clinic. This is an excellent statement of a conservative sort concerning the various aspects of the problems of criminal behavior, if rather short on the treatment aspects.

WENDELL MUNCIE

BRAINSTORM. *By Carlton Brown. Farrar and Rinehart, New York and Toronto.* $2.75. 302 pp. 1944.

This is an autobiography—the story of a psychosis and efforts at treatment and recovery. From the scientific standpoint it would seem that the diagnosis of dementia praecox, made at the first psychiatric hospital, was quite wrong. The account appears full of affective elements of elation, and the manner of recovery would point to the same diagnosis. What good this kind of a book does is hard to see. It is hard to see also where the author's enthusiasm for psychoanalysis comes from, because it appears not to have been used in his case at all. Maybe that is why he is enthusiastic about it, as the methods that were used seem to have contributed so little to his recovery.

WENDELL MUNCIE

FREUD: *Master and Friend. By Hanns Sachs. Harvard University Press, Cambridge, Mass.* $2.50. 195 pp. 1945.

This is one of those personal volumes concerning Freud by one of his earliest pupils. As such, it has a great deal of intimate material in it bearing on the character and work of Freud. These are days when books of this sort may be expected to multiply, and it is all to the good, since it will all help in the final evaluation of Freud as a man, as an artist, and as a scientist. This reviewer can only point with wonderment to the excessive degree of self-effacement, humility, almost of embarrassment which the author shows at inscribing his data. This remains unexplained, and since the book is about Freud and not about Sachs, maybe we have no right to insist on an explanation.

WENDELL MUNCIE

THE NEUROLOGIST'S POINT OF VIEW. *Essays on Psychiatric and Other Subjects.*

By I. S. Wechsler. L. B. Fischer, New York. $3.00. 251 pp. 1945.

This is one of those personal books in which the author, a noted neurologist, allows himself the privilege of discussing a great many things having little or nothing to do with neurology. In fact, the eleven chapters of this book have to do with various aspects of the racial psychology of the Jew and its bearing on the well known facts of nervous disorders among the Jews, the psychology of anti-Semitism, a sympathetic account of Zionism, together with observations of neurotic conflict from the psychoanalytic standpoint, a critical appreciation of Freud, a review of Moses and monotheism, a brief history of psychiatry, a rather defeatist chapter on the problem of mental disorders from the neurologist's point of view, and also one on the legend of the prevention of mental disorders.

Patently, these chapters are not all of the same importance from a scientific standpoint. I read hopefully the chapter, "Nervousness and the Jew; an Inquiry into Racial Psychology," in the hope of coming to a better grasp of this problem. I am not impressed with the author's argument that the Jew exhibits in a greater degree the struggle exhibited by everyone, that of conflict between reality and idealism. Also I feel fairly certain that even among Jews there are a great many people who would take issue with his statement that "a strong sense of reality alone would long ago have taught him (the Jew) the advantages of giving up his identity. It would long ago have convinced him of the egregious folly of being a Jew. But he could not yield his identity unless he consented to commit spiritual suicide. He could not, if he would, because his inner strivings, his ideal ego, stood sentinel over him. Those Jews whose greater sense of reality made them accept the advantages of non-Jewish life did so at the expense of their ideal ego and their cultural heritage."

In the chapter on "The Legend of the Prevention of Mental Diseases," the author makes some statements which need to be challenged. He says, "It is evident, therefore, that the ability to treat a disease, even to the point of cure, does not altogether depend on the knowledge of its cause, but it is absolutely certain that no diseases can be prevented unless its specific cause and the manner of its transmission are definitely known." As to this conclusion, it is almost certain that the old time syphilologist, who knew nothing about the specific cause and the manner of transmission of syphilis, knew enough to be able to teach that sexual continence was a practically sure preventive. Similarly he says, "There are diseases which we know neither how to prevent nor how to cure. . . .The same applies to most tumors of the brain, nearly all the psychoses, and a great many other conditions." This is palpably not the case, as any psychiatrist can avow. In the same chapter he launches a diatribe against psychiatry, accusing it of a "groveling worship of formulas, an infatuation with fetishes of words. New shibboleths are coined, and the illusion is created that new facts have been discovered. Abracadabra is uttered with the positiveness of magicians, and presto, new wisdom springs fullgrown out of nowhere. We solemnly speak of a psychobiologic or a psychodynamic approach, but if Socratically pressed for a real answer to the question: what is meant by psychobiologic or psychodynamic? we are in truth compelled to say, why—psychobiologic and psychodynamic." This is a serious charge and one which may be sustained only in part. It is true that there are people who do just that, but the great author of the terms psychobiology and psychodynamics as applied to psychiatry should be excepted from this general diatribe. If the author had undergone any serious training in psychobiologic or psychodynamic psychiatry, he would not be able to utter such bitter words. He would have realized that these are words not coined to cover up ignorance, but coined to furnish an incentive to discover more and more facts.

Already this review has given more attention to the book than it deserves. As stated in the first paragraph it is a personal document, and as such it is interesting. The author as a neurologist clearly has his limitations when he starts talking about things psychiatric. What he has to say is interesting and provocative, sometimes helpful and sometimes simply defeatist. All in all, it will repay reading.

WENDELL MUNCIE

CONSCIENCE AND SOCIETY. *A Study of the Psychological Prerequisites of Law and Order.*

By Ranyard West. Emerson Books, Inc., New York. $3.00. 261 pp. 1945.

An American edition of the important book previously reviewed in this journal (vol. 19: 172. 1944).

MIND EXPLORERS.

By John K. Winkler and Walter Bromberg. The World Publishing Company, Cleveland and New York. $1.00. 378 pp. 1939.

A reprint of the book previously reviewed in this journal (vol. 15: 260. 1939).

HUMAN BIOLOGY

THE SCIENCE OF MAN IN THE WORLD CRISIS.

Edited by Ralph Linton. Columbia University Press, New York. $4.00. xiv + 532 pp. 1945.

In his preface the editor calls attention to human inertia by stating that "it usually takes about a generation for the new discoveries and techniques of one science to become a part of the regular working equipment of other sciences. It takes considerably longer for such findings to become familiar to the layman and to exert any significant influence upon his thinking. The present book is an attempt to shorten this time interval." The rapidly growing science of man can and must be of aid in planning the new world order by showing the potentialities and limitations of the human material with which and for which the future is to be built. This collection of essays is not supposed to represent a condensation of all accumulated anthropological knowledge, but rather a report on recent advances in this science that deals only lightly with the facts and findings already well established. Thus, it is taken for granted that the reader "knows and accepts the basic facts of man's origin and evolution" and that he is fully aware of the issues of Negro and Jewish minorities. On the other hand, it discusses at length, e.g., the pressing problems of the American Indian, of which so few people are aware.

In the first chapter the editor presents authoritatively the Scope and Aims of Anthropology, which is defined simply as "the science of man and his works." Here it is pointed out that "anthropology has concentrated its interest upon a single organism, man, and has tried to understand all sorts of phenomena as they affect him." Thus it is a synthesizing science, split according to its definition, man and his works, into physical and cultural anthropology, aligned with the natural and the social sciences respectively.

The second contribution, entitled "Society and Biological Man," is by H. L. Shapiro. This brief review of the most significant biological facts relating to mankind deals with race-crossing, selection, environmental effects, etc., as far as general statements can be made to-day. This able, but necessarily fragmentary, outline of human biology demonstrates how incomplete our knowledge in this science still is, and how helpful it could be for the biological welfare of our populations.

The third paper, by W. M. Krogman, discusses that never-ending, controversial topic, "The Concept of Race." After emphasizing the fact that all men are far more alike than different, that all physical characters have a genetic basis, and that group differences exist chiefly in regard to the relative frequency of particular features, the author leaves no doubt of his conviction that differentiation has occurred and continues to occur in mankind. There follows a condensed presentation of the main physical characteristics of the three great human stocks, White, Yellow, and Black, and of the classification and probable interrelationships between the many minor groups. It is made clear that human races cannot be defined precisely and that "they are loose aggregates which precipitate out only en masse."

A further chapter, by O. Klineberg, on "Racial Psychology" leads to the blunt conclusion "that racial differences in psychology have not been—and perhaps cannot be—demonstrated." This, it appears, is largely due to the fact that no satisfactory tests of intelligence have as yet been devised which are entirely free of cultural or environmental influences.

Still another paper of great biological interest deals with "Population Problems." It is written by the botanist, K. Sax. Since this lucid contribution concerns itself with the latest and perhaps most consequential evolutionary specialization of man—the reliance on numbers of individuals—it deserves particularly to be widely read. Even though man has existed for a very long epoch, the world's population did not reach a one billion total until some time in the first half of the nineteenth century. In an additional one hundred years it nearly doubled, and in seventy years more it is expected to become doubled again. As the author states simply: "this. . .creates problems which lead to economic and military strife." He might have added that it cries for far-sighted social planning. Of the many interesting data in this chapter the following few examples may be quoted here: "During the past three hundred years the white population has increased about 700 per cent, the Yellow-Brown races a little more than 200 per cent, while the pure Negroid population has increased only about 100 per cent." No wonder that "of the people of European origin there are nearly half as many in other parts of the world as there are now in Europe." The author's comments and conclusions, though not new, cannot be stated too often, as they are rarely heeded. For instance: "Man has made little conscious effort to solve the problem of population growth and distribution." "If the birth rates are not restricted the population can be held in check only by a high death rate. This condition prevailed until modern times and still persists in most of Asia." "Control of the birth rate is essential in most parts of the world, and is prevalent in all countries which have attained a high standard of living." That the latter standard can become available to all seems most doubtful to the author in the present state of man's social evolution.

The sixteen remaining contributions to this volume are all devoted to cultural, social, economic, and political or administrative topics and as such are not to be reviewed in a biological journal, even though they are of great and timely interest to every one who shares the faith that the future of man has to be built on a Science of Man.

A. H. SCHULTZ

A SCIENTIFIC THEORY OF CULTURE *and Other Essays. By Bronislaw Malinowski; preface by Huntington Cairns. University of North Carolina Press, Chapel Hill.* $3.00. ix + 228 pp. 1944.

Of the three posthumously published essays of the late Bronislaw Malinowski, who died three years ago at the age of 58, *A Scientific Theory of Culture* and *The Functional Theory* are almost identical in content, while the third, on Sir James George Frazer, containing personal memories and an analysis of Frazer's work from the functionalist point of view, transcends the scope of the preceding studies.

Malinowski is the outstanding representative of a whole period in anthropological thought which followed evolutionism and diffusionism, and which he called "functionalism." Mostly through his influence methods of field work have been greatly changed and improved, and a new approach to cultural anthropology has become predominant. Malinowski did not invent "functionalism," just as neither Marx discovered the "class struggle" nor Freud the "unconscious." But through his dynamic and rich personality, and a somewhat reckless and single-minded persistence, he has forced this principle upon his contemporaries, as Marx and Freud had done in their respective fields.

To Malinowski, the study of culture is the real meeting-ground of all branches of anthropology. Institutions are the legitimate isolates of cultural analysis, and are to be understood as culturally colored responses to a few basic biological needs. It is obvious that Malinowski's notion of function is closer to the biological than to the mathematical meaning of the term. His Functionalism is to a large extent the introduction of behaviorism into anthropology. Like behaviorism, it represents at once a great progress, and remains highly unsatisfactory as a total answer to the problem in question. It simply cannot be proved that every element of culture is functional in a biological sense, nor even that every element is functional at all. Functionalism which consciously omits every kind of historical reasoning is unable to explain the perplexing fact why the same basic biological needs receive such a multiplicity of answers in different cultures. That Malinowski eventually became aware of the latter problem can be seen in his third essay, where he tries to make a synthesis between his approach and the evolutionary and historical point of view.

It is impossible to go here into a detailed discussion of all of Malinowski's definitions, though probably more than the reviewer will be in definite disagreement with many of them, e.g., those given for "science," "politics," or "economics." In order to be just with the author, it is necessary to realize that not only his attempt at a general anthropological theory, but all similar attempts suffer from very serious shortcomings, and general anthropological theory is still in the stage of gestation. The relative absence of terminological acrobatics and the earnest search for the truth are very praiseworthy aspects of these essays. Though the posthumous publication of books has always seemed to me a highly problematic enterprise, it is on the other hand very practical to have Malinowski's theoretical system now available in a book, while heretofore it could only be found in his contributions to the Encyclopedia Britannica, the Encyclopedia of Social Sciences, and various articles and forewords. Malinowski had not fundamentally changed his point of view, when we compare these essays with the earlier ones, but he had become considerably more tolerant and inclined to incorporate the results of other schools and researchers. While he still did not find any place for physical anthropology in his system, the mellowness of age induced him to make even such extreme overstatements as, e.g., that Ellsworth Huntington had proved something "beyond doubt and cavil." The book is indispensable reading for all those who are interested in anthropological theory.

ERWIN H. ACKERKNECHT

THE CULTURAL BACKGROUND OF PERSONALITY. *By Ralph Linton. D. Appleton-Century Company, New York and London.* $1.50. xix + 157 pp. 1945.

Studies on the relations of personality and culture have brought about closer collaboration between anthropologists, sociologists, and psychologists. Ralph Linton of Columbia University, who has been one of the pioneers of these cooperative efforts, and who has consistently dealt with different aspects of the problem in his earlier books, sketches a conceptual framework for the systematic study of culture, society, and personality in this little volume, based on five lectures delivered at Swarthmore College in 1943.

In the study of culture Linton has introduced the useful differentiation between overt and covert aspects of culture, and between real culture, the culture construct, and the ideal pattern of a culture. In the analysis of social structure, he emphasizes the notions of status and role. As to personality, the responses forming it are divided into specific and general ones. The latter are highly dependent on the value-attitude systems of the individual. Though biological factors undoubtedly contribute in the formation of personality, especially in small, isolated, intermarrying communities, personality is mainly formed by culture in the process of child rearing, and by being provided throughout life with new cultural models for specific responses.

The author has been strongly influenced by behavioristic methods, but largely transcends this rather limited approach, as, e.g., in his excellent statements on psychic needs and the double role of the individual as object and subject of society. In spite of the somewhat algebraic character of the small book, which makes it sometimes no easy reading, it will undoubtedly be

highly appreciated by all interested in the problems of culture and personality for its lucid analysis and highly stimulating qualities.

ERWIN H. ACKERKNECHT

THE HOPI WAY.

By Laura Thompson and Alice Joseph. Foreword By John Collier. University of Chicago Press, Chicago. $3.00. 151 pp., 24 plates. 1944.

This is a work done in cooperation between the Indian service and the Committee on Human Development of the University of Chicago. Its purpose is to get behind an Indian way of life; to try to avoid our preconceived ideas about "the Indian," and to try to find what individuals within a group are like, and thereby to gain some idea of how the group functions. From this base it is the hope of the Indian Service to build better administrative programs aimed at doing the maximum good for the tribes and the minimum harm.

The work is intended to be holistic in its approach. The natural environment, economy, history, and social structure of the group, as well as the life cycle of the individual is presented. The major emphasis falls on the analysis of the psychological tests given the Hopi children (Rorschach, Thematic Apperception, and six others). A series of selected individual analyses are presented to illustrate different types of adjustment within the Hopi social group.

The work tends to show a very well adjusted group of peoples existing in a difficult natural environment and maintaining nearly intact their ancient social culture. It is an excellent study. If any critical comment were called for, it would merely be that the lengthy testing had brought to light disproportionately few new insights into Hopi culture. In the sharpening of our understanding, however, it is a considerable contribution. Psychologists should be particularly interested.

GEORGE F. CARTER

THE ALEUTIAN ISLANDS: THEIR PEOPLE AND NATURAL HISTORY (*With Keys for the Identification of the Birds and Plants*). *Smithsonian Institution War Background Studies Number Twenty-one.*

By Henry B. Collins, Jr., Austin H. Clark, and Egbert H. Walker. Smithsonian Institution, Washington. 25 cents (paper). iv + 131 pp.; 21 plates. 1945.

This is a brief presentation of the history, ethnology, and natural history of the Aleutian Islands. It is aimed primarily at the service men stationed there and is designed to aid them in getting to know the area.

The introductory section on "The Islands and Their People" is compact and interestingly written. There is a description of the geography and a good accompanying map. The history of the discovery and exploitation of the islands is given. The sketch of the people is brief and avoids technical details that the non-anthropologist would struggle over.

The section on "Animal Life of the Aleutian Islands" stresses the rich bird life of the area. The fishes and sea mammals are also described. Appendices give a list of common mammals, common birds, and a key to the recognition of the birds of the area.

The third section, "Plants of the Aleutian Islands," sketches the plant life found there. It is supplemented by appendices listing the plants, and giving a key to the recognition of the plants of the islands.

GEORGE F. CARTER

EUROPE: *An Atlas of Human Geography.*

By Marthe Rajchman. William Morrow and Company, New York. $2.00. 120 pp. 1944.

This is a book of maps, charts, graphs, and diagrams assembled with the aim of providing background for understanding the problems of Europe. The data and the maps have been culled from a variety of sources. The assemblage of maps is useful for gaining a quick view of some aspects of Europe. A great variety of subjects, from simplified climates and linguistic and national distributions to national income and wartime population movements are shown.

There is an accompanying text which is neither very convincing as to the author's geographic expertness nor very significant. It does not seem correct to call Europe the densest human agglomeration in the world; *global* connections with Europe should surely consider more than the North Atlantic routes to Europe; few will believe that Europe has the best climate in the world for human activities; etc.

GEORGE F. CARTER

FOOD, WAR AND THE FUTURE.

By E. Parmalee Prentice. Harper and Brothers, New York and London. $2.50. xii + 164 pp.; 5 plates. 1944.

The author of *Food, War and the Future* writes with an abundance of literary references in a style of considerable charm. He attacks with vehemence and defends with ability. His first chapter raises the specters of a world population increasing in unlimited fashion, of want returning, of the tide of yellow Asia rolling over the western continents. With neo-Malthusian enthusiasm he discusses the recent "Century of Grace" the West has had, in its relative freedom

from want, for the first time on a large scale in human history. The predictions of returning disaster, however, seem to be based on the assumption of constant rates of increase in population and to ignore the sounder and more conservative demographic estimates to be found, for example, in such studies as those in *World Population in Transition*, reviewed elsewhere in this issue.

Prentice places his hope for future freedom from want on the achievements of genetics applied to plant and animal breeding. At this point we meet another of the author's *bêtes noires*—the prevailing regard for pedigreed pure-bred stock in the improvement of dairy cattle. He demolishes the myth of the early foundation of the well-known breeds, and is devastating in his criticism of the professors in agricultural colleges who have taught and endorsed these historical fairy tales. He does the pure breeds an injustice, however, in failing to cite the data that show how much of an improvement in milk production has actually accompanied the development of the pure breeds. He is perfectly right, of course, in emphasizing the complete superiority of the progeny test as a measure of breeding worth.

Prentice next proceeds to discuss the relation of human freedom to want, under the heading of "Democracy in America." Here he reveals his basic point of view more clearly. It is that of the nineteenth century liberal transmuted into laissez-faire individualist and isolationist. The transition is subtle. "American democracy is found in the inalienable and equal rights secured to all persons by the constitution." "There can be no freedom and no democracy without effective limitation upon government, and government is no friend to limitations upon its power." "It is very disturbing, indeed. . .alarming, to think of the sudden rise in Great Britain and America of the demand for a protected life." "No government is productive." "At the worst America can not be defeated. Without American supplies. . .Great Britain is helpless, but America would be stronger than ever were she to withdraw from Europe and hold her own frontiers free from the burden of defending other nations." "The Beveridge plan is at best a temporary expedient. It is not a living system under which society can endure." He speaks approvingly of German rule over central Europe as the best ultimate solution of the European problem. "The freedom we intend to preserve is the constitutional freedom which brought the American era, nothing less and nothing different"—and therefore nothing more!

This is a most interesting book, stimulating in many respects—this must be said in spite of one's severe criticism and perhaps fundamental disagreement as to ultimate conclusions.

BENTLEY GLASS

WORLD POPULATION IN TRANSITION. *Annals of the American Academy of Political and Social Science. Volume 237, January, 1945.*

Edited by Kingsley Davis. The American Academy of Political and Social Science, Philadelphia. $2.50 (cloth); $2.00 (paper). viii + 257 pp. 1945.

No more comprehensive and varied a survey of world demographic problems could be wished at the present time than this special number of *The Annals*. It begins with a general article by the editor on "The World Demographic Transition." There follows a section of regional studies, on the United States (Philip M. Hauser and C. Taeuber), South America (Halbert L. Dunn, Hope T. Eldridge, and Nora P. Powell), Central and Caribbean America (Alberto P. León and Alvaro Aldama C.), postwar Europe (Dudley Kirk), Russia (M. Kantorovitz Gordon), Japan (Irene Taeuber and Edwin Beal), China and southeastern Asia (Warren Thompson), the British Dominions overseas (Enid Charles), the Mohammedan world (Ernest Jurkat and Louise Kiser), and primitive areas (S. F. Cook). The second section deals with various aspects or factors of population change, and contains: Trends, Determinants, and Control in Human Fertility, by P. K. Whelpton and Clyde Kiser; Trends in Longevity, by Louis I. Dublin and Alfred Lotka; Infant and Maternal Mortality in the Modern World, by Jacob Yerushalmy; The Trend Toward an Older Population, by John Durand; Sickness and Health—Their Measurement, Distribution, and Changes, by Selwyn Collins; Pressures and Barriers in Future Migration, by Edward Hutchinson and Wilbert Moore; Relocation of Europeans, by Imre Ferenczi; Population and Per Capita Income, by Joseph Spengler.

The volume ends with an excellent summary of the Issues of Population Policy, by Frank Lorimer. Not only every biologist, but every alert citizen is urged to study this collection of papers thoroughly. No intelligent understanding of world affairs can fail to take into account the problems it discusses.

BENTLEY GLASS

BIOMETRY

EINFÜHRUNG IN DIE WAHRSCHEINLICHKEITSTHEORIE. *By Erich Kamke. (S. Hirzel, Leipzig); J. W. Edwards, Ann Arbor, Michigan.* $3.85. vii + 182 pp. (1932); 1944.

The publication of an American edition of this book is under the authority of the Alien Property Custodian. The book is not a translation but is a lithoprinting of the original edition.

The author refers in the foreword to the increased importance of probability theory to scientists, because of the recognition by modern physicists of its role in

their science. He undoubtedly has this group in mind in his presentation.

The probability of an event, according to Kamke's concept, is the limit of the ratio of the number of occurrences of the event to the number of trials, as the number of trials is increased indefinitely. The book presents under this definition the classical theory of the fundamental theorems of addition and multiplication and some of the famous problems on games of chance in Part I, and then takes up errors of observation, the Bernoulli and Lexis theories, and the normal curve in Part II.

The definition of probability as the limit of the relative frequencies in this presentation is that of Von Mises and certain other recent writers on probability. Although it is desirable to have inaccessible foreign publications made available, this particular one adds so little that is new that one might well question the importance of publishing it under the circumstances involved in the copyright situation during the war.

MARGARET MERRELL

STATISTICAL METHODS FOR RESEARCH WORKERS. *Ninth Edition, Revised and Enlarged.* *By R. A. Fisher. Oliver and Boyd, Edinburgh.* 16s. xv + 350 pp. 1944.

Except for minor changes, this is the same work that has long since become standard in its field in earlier editions (see review in this journal, vol. 17: 189. 1942). A new section (57.3) has been added on the testing of homogeneity of evidence used in estimation.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

LABORATORY METHODS OF THE UNITED STATES ARMY. *Fifth Edition.* *Edited by James Stevens Simmons and Cleon J. Gentzkow. Lea and Febiger, Philadelphia.* $7.50. 823 pp.; 8 plates. 1944.

The well printed, well bound fifth edition of this manual was prepared—like the first edition, which appeared during World War I—to describe "practical methods for use in the medical and sanitary laboratories of the Army." More specifically, it was designed "for the large numbers of officers and technicians who entered the Army and assumed responsibility for its enormously expanded laboratory services." Sufficient theory is included in its more than eight hundred pages to make possible intelligent and even versatile work. This manual does not claim to have everything about everything, but the material covered is so extensive that it is difficult to imagine any kind of a biological laboratory in which it would not be an extremely valuable reference.

Part I—Clinical Pathology, includes twelve chapters ranging from Kidney Function Tests and Hormonal Tests for Pregnancy to chapters on Human Blood Groups and the Diagnosis of Syphilis. Part II—Chemistry, with eight chapters, runs from General Chemical Technic and Blood Chemistry to Toxicological Methods and Useful Recipes. Part III—Mycology. Part IV—Bacteriology, includes chapters on General Technic, Classification, Bacteria of Medical Importance, Bacteriologic Examination of Milk, Water and Sewage. The remaining seven portions of the manual cover respectively, Part V—Rickettsiae and Filtrable Viruses, Part VI—Protozoology, Part VII—Helminthology, Part VIII—Entomology, Part IX—Pathology, Part X—Veterinary Laboratory Method and Part XI—Statistical Methods.

In general, the twenty-six contributors have turned out a remarkably competent and up-to-date piece of work. As a rule the explanations are clear, the directions adequate and specific. The range of material is such that it will be of practical use both to the architect turned medical corpsman and to the veteran of the laboratory or the specialist with three degrees after his name.

Any manual bearing the proud title of this one should be second to none. The urgency of the need is too great to tolerate either lack of adequacy or second-best methods. Obviously, no one individual is competent to judge every section of a volume of this scope. Nevertheless, anyone with even a moderate amount of laboratory experience cannot fail to be distressed at the evidences of either haste in preparation or lack of experience in writing directions that are to be followed by men or women working on their own. Examples are to be found in several parts of the book.

The section on medical entomology, admirable in most respects, unaccountably recommends a compound microscope for use in the identification of insects, when a binocular dissecting microscope is many times superior. The keys could be improved. The first bedbug brought in for identification (surely not an unlikely contingency) will stall early in the first key. The key for the fleas depends on technical terms not always explained. The figures of fleas could both be improved in clarity and made more relevant. The several pages discussing families and genera of insects of medical interest are rendered worthless from any practical point of view by a lack of figures, keys, or diagnostic descriptions. Sergeants may be grateful to learn that the Anthomyiidae includes, *inter alia*, the "latrine fly," *Fannia scalaris*, but there is nothing in this manual that would enable a full colonel to recognize one when he saw it.

The directions for the diagnosis of rabies in a dog are

unnecessary if the technician already knows how, inadequate if he does not. These directions call for staining parts of the hippocampus major for Negri bodies. Detailed instructions are given for preparing and using both Seller's and Mann's stains. But what is the unfortunate man to do who has forgotten what and where the hippocampus major is? There is neither a diagram nor a description of it. Neither is there a figure or description of a Negri body. It would seem desirable to enlarge on the bare statement that Negri bodies should appear red, and to include, as many manuals do, a word of caution about the need for wearing rubber gloves and even goggles in handling the head and removing the brain of a dog suspected of being rabid.

The Arneth leucocyte count and Schilling's modification of it are discussed at some length, but without either diagrams or definite percentages, so that from what is given here it would not be possible to make such a count. Directions are given for removing various stains from clothing, including blood stains, but we fail to find any mention of serological tests to determine whether or not such a stain was made by human blood—sometimes a question of great importance. Directions for embalming are given with a wealth of detail that would have delighted Edgar Allen Poe. Everything is there,—everything, that is, except the formula for embalming fluid. (This is no professional secret, as the formula approved by the American Funeral Director's Association is published in the Scientific American Cyclopedia of Formulas.) The bacteriological standards for the purity of milk are incompletely given. The standards for cream are so vague they are virtually meaningless. The section on the fungi seems curtailed. The section on the microscopic study of tissues has no instruction for one of the trickiest and most necessary of laboratory chores, sharpening a microtome knife.

In short, notwithstanding all the virtues of this manual, and they are many, there are good reasons for hoping that no one will have to wait a decade for the next edition, as we did for this one. A sixth edition, remedying the deficiencies of the present volume and incorporating the results of experience with DDT and all the other new materials and new methods, would win universal acclaim.

GAIRDNER MOMENT

SEEING THE INVISIBLE. *The Story of the Electron Microscope.*

By Gessner G. Hawley. Alfred A. Knopf, New York. $2.50. xv + 195 + ix pp. 1945.

Here is a book written in a simple, almost elementary style that succeeds in making clear the principles of operation of the electron microscope, its powers, and its disabilities, especially in dealing with biological objects. The history of the instrument is recounted and its achievements to date are amply illustrated. The author commits a few gross biological errors, attributing the action of serums and vaccines to bacteriophages, for example, and confusing photosynthesis with protein synthesis. It is amusing to read that genes are carried in "red-colored rod-shaped envelopes in the sex cells." But on the whole, the account is good, and should be intelligible to anyone with enough intellectual curiosity to pick up and read a book about the electron microscope. The outstanding value of the book consists in its abundant and well reproduced illustrations. For these alone most biologists will want to own *Seeing the Invisible.*

ERRATUM

In the June number of THE QUARTERLY REVIEW OF BIOLOGY (page 186), the price of the Bulletin of the National Research Council Number 109, "Inadequate Diets and Nutritional Deficiencies in the United States," is incorrectly stated. The price is 50 cents.

JOHN CAMPBELL MERRIAM

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